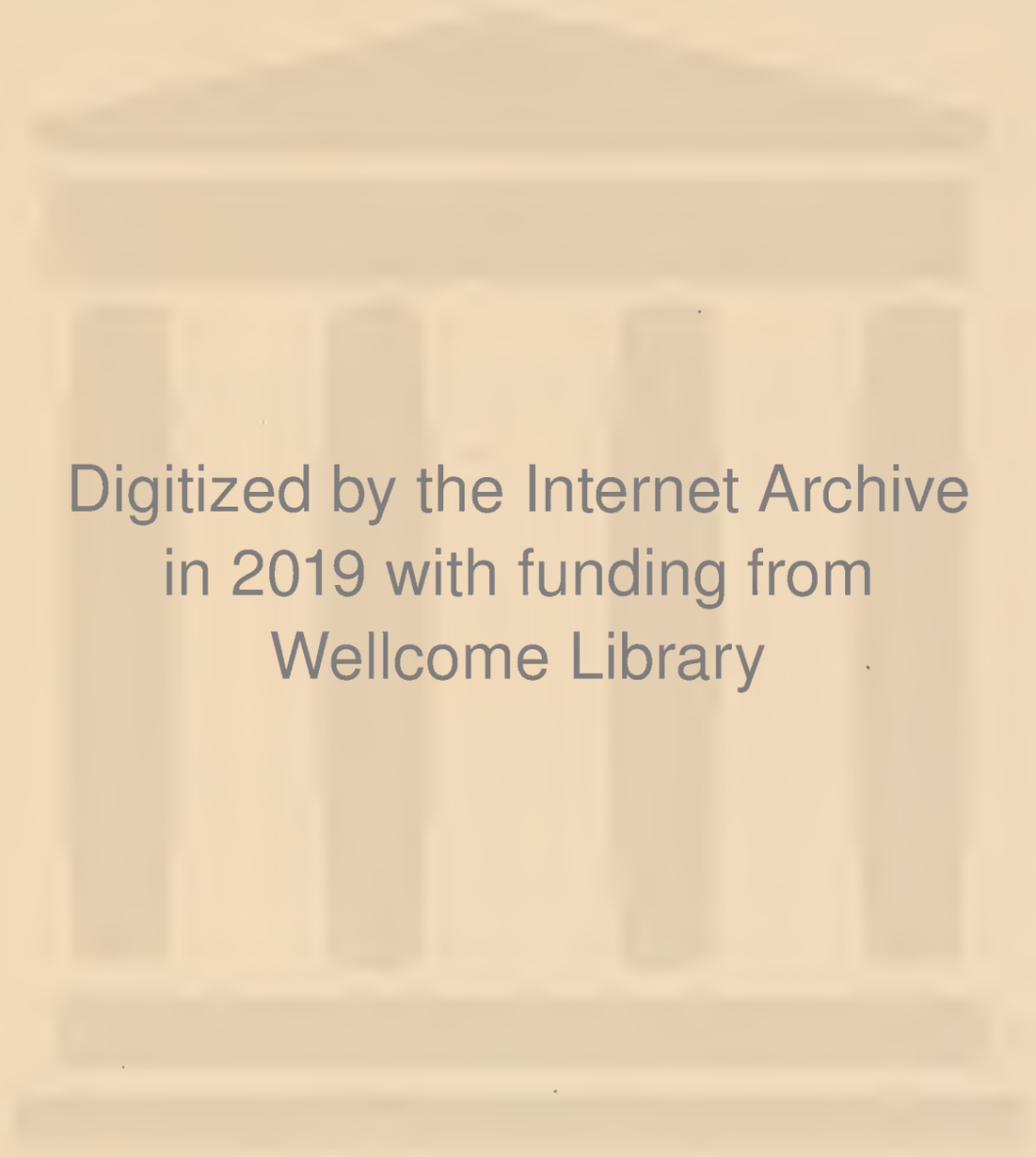
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METEOROLOGICAL SOCIETY OF LONDON

THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND COMMERCE.

By RICHARD TAYLOR, F.L.S.

MEMBER OF THE ASTRONOMICAL SOCIETY OF LONDON, OF THE METEOROLOGICAL SOCIETY; AND OF THE ROYAL ASIATIC SOCIETY OF GREAT BRITAIN AND IRELAND.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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ALBERT FLAMMANG

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ERRATA:

Page 211, line 27, supply " and tables " after instrument.
Page 212, line 4, *for* cos. D *read* cos. δ.

LONDON

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st JULY 1825.

I. *On the Laws of the Condensation and Dilatation of Air and the Gases, and the Velocity of Sound.* By J. IVORY, *Esq. M.A. F.R.S.*

IN a letter inserted in this Journal for June 1824 I have already hinted at the connexion between the pressure, density, and temperature of air at different elevations in the atmosphere, and of air which is dilated while it retains all its heat. The two cases have at least this in common, that in one and the other the elasticity and temperature depend entirely upon the density. In an atmosphere *in equilibrio*, which preserves its constitution, there is but one temperature and one pressure for every proposed density; and when air is dilated without acquiring heat from other bodies, and without dissipating any of its own, there is but one temperature and one pressure answering to every increase of volume. A parcel of air finds its place in the atmosphere by enlarging or contracting its bulk till its temperature and elasticity are *in equilibrio* with the surrounding mass. In both the cases compared a change of volume or density is the sole cause of a variation of pressure and temperature. There is thus a great analogy: but we must not hastily conclude, as Mr. Dalton has done, that there is an exact identity. A little reflection will show that there are circumstances in the one case which do not exist in the other. What is the arrangement of air in the atmosphere that would be produced by the main cause we have mentioned, and how far that arrangement would be affected by the particular circumstances that modify the operation of that cause, is an interesting subject of research. In the present state of the inquiry into the constitution of the atmosphere, the whole power of dynamical science seems to be exhausted; and we cannot reasonably hope that much further progress will be made, without extending our knowledge of the physical pro-

perties of air. An investigation instituted on the principles I have endeavoured briefly to explain, promises at least some degree of success.

But a preliminary investigation seems necessary. In what manner do the elasticity and density of a mass of air depend upon the heat combined with it in a latent form, and the heat of temperature sensible to the thermometer? It is evident that the latter quantities completely ascertain the condition of a given body of air. If both of them remain constant, no change can take place in the air's elasticity or density. If both, or either of them vary, an alteration of the elasticity or density, or of both together, will necessarily ensue. In the present communication I confine myself to this preliminary inquiry.

We have no knowledge, in any instance, of the absolute quantities of the heat of combination, or of the heat of temperature; we can reason only about the differences that occur in the same body placed in different circumstances. For this reason it is always necessary to refer the air, or gas, under consideration, to some original or standard condition. I shall suppose that the elasticity and density are each originally equal to unit; and I shall put p for the relative elasticity, or pressure, and $\varrho = 1 \pm \omega$ for the relative density, in any other state. Thus if h be the barometric pressure of the original body of air, and h' the like pressure when the air has varied in any manner, then $\frac{h'}{h} = p$; and if D and D′ be the densities in the same two states, then $\frac{\mathrm{D}'}{\mathrm{D}} = \varrho = 1 \pm \omega$. The original temperature being τ, and the difference of temperature θ, the actual temperature will be $\tau + \theta$. If α denote the dilatation for one degree of the thermometer, we have this fundamental equation between the quantities defined, viz.

$$p = \varrho \times \frac{1 + \alpha\tau + \alpha\theta}{1 + \alpha\tau};$$

and I shall call the expression by which ϱ is multiplied the factor, or the function, for temperature. Lastly, I shall denote by $\pm i$ the degrees of the thermometer evolved or absorbed, when air or a gas passes from the original to any other state; reckoning positive the heat which becomes sensible to the thermometer when air is condensed, and negative the heat which disappears when air is dilated.

1. Taking the air at the surface of the earth as the standard for comparison, let a given mass of it be contained in a close vessel; then let the dimensions of the vessel be suddenly enlarged, allowing the air within to dilate till the density is diminished

diminished from unit to $1 - \omega$. When the rarefied air has resumed the general temperature on the outside, it is obvious that $1 - \omega$ will likewise represent the elasticity within the vessel. But at the instant of the rarefaction, the heat absorbed by the dilated air will produce a certain degree of cold, and consequently a diminution of elasticity. Taking the letters τ and i in the meaning we have explained, i will denote the depression of the thermometer within the vessel at the instant of the rarefaction; and the elasticity of the dilated air will, at the same instant, be equal to

$$(1 - \omega) \times \frac{1 + \alpha\tau - \alpha i}{1 + \alpha\tau}.$$

If the dimensions of the vessel be suddenly lessened, the density of the confined air will be increased to $1 + \omega$, and heat will be evolved. The elasticity within the vessel at the instant of condensation will be equal to

$$(1 + \omega) \times \frac{1 + \alpha\tau + \alpha i}{1 + \alpha\tau};$$

but it will change to $1 + \omega$ in a short moment of time when all the heat of condensation is dissipated.

In these experiments the sole cause of the heat i is the enlargement or diminution of the volume, or the variation of the density, of the mass of air. It has no dependence on the external temperature τ. If the temperature be varied, the elastic force of the air within the vessel will be proportionally augmented or diminished; but the combined heat i will not be affected by the heating or cooling, so long as the containing vessel remains unchanged in its dimensions. If the dilated or condensed air be reduced to its original volume, the heat of combination will be disengaged and become sensible to the thermometer, or it will be absorbed and disappear from that instrument; and this will be the case whether the restoration of volume be made at the original temperature τ, or at any other temperature greater or less than τ. The volume of a mass of air or its density, and its heat of combination, are two things inseparably united; and no change can be made in one without a consequent alteration of the other. In the language of the Mathematics, the one is a function of the other; and it will readily appear from what has been said, that the density of a mass of air may be expressed in terms of its heat of combination by an equation of this form, viz.

$$\varrho = \varphi\left(\frac{1 + \alpha\tau + \alpha i}{1 + \alpha\tau}\right);$$

the function φ being liable to no limitation, except that it must be equal to unit when $i = 0$, in order that the density may

resume

resume its primitive state when the heat of combination vanishes.

But although the density of a mass of air depends solely upon the heat combined with it in a latent form, the temperature will be affected by the variations of density, as well as by the heat flowing from foreign sources. It is lessened, at least for a moment of time, by all the degrees absorbed when air is rarefied, and increased, for a moment, by all the degrees disengaged when air is condensed. Let T denote the actual temperature, and θ the heat received from extraneous sources; then,

$$T = \tau + i + \theta.$$

This formula includes all the possible sources of temperature, but the difference between the quantities represented by i and θ must be carefully marked. In the circumstances that generally occur in nature, θ is a constant and perpetual current of heat keeping up the temperature to a determinate level. On the other hand, i is sensible only while the air is actually undergoing changes of volume, and is soon lost in the general influx of heat after the density becomes constant. When this happens, the temperature is independent of the heat of combination. If we suppose that air changes its volume and retains the whole of its absolute heat while it is exposed to no extraneous temperature, then $\theta = 0$. In the atmosphere i and θ are quantities of a like description, always comparable and dependent on one another. These two cases are, if I mistake not, the only ones that have hitherto been observed in which the elasticity, the density, and the temperature of a mass of air are all ascertained when one is found, and consequently in which all of them are functions of the heat of combination.

The known laws of elastic fluids will now furnish these equations, viz.

$$\varrho = \phi\left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)$$

$$p = \phi\left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right) \times \frac{1+\alpha\tau+\alpha i+\alpha\theta}{1+\alpha\tau} \qquad \text{(A)}$$

$$T = \tau + i + \theta.$$

2. We must next endeavour to determine the form of the function ϕ. This may be accomplished by means of certain experiments made by MM. Clement and Desormes and MM. Gay-Lussac and Welter. The two first-mentioned philosophers filled a close vessel with air, varying in no respect from the external air, the height of the barometer being equal

equal to h. A portion of the air was now extracted from the vessel, and, allowing the loss of temperature caused by the rarefaction to be restored, the pressure within the vessel was observed equal to h'. A communication was next opened between the confined air and the atmosphere; and the barometric pressure in the vessel having increased in a very short moment of time to the height h the same as on the outside, the communication with the external air was instantly shut. Lastly, the heat of the condensation caused by the rushing in of the external air being entirely dissipated, the barometric height within the vessel was again observed equal to h''. It is essential that the temperature and barometric pressure of the atmosphere remain unchanged during the time of this experiment.

We shall most readily understand the conclusions to be deduced from the experiment, if we note separately the circumstances occurring at the different epochs during the course of it.

1st. The initial pressure, density, and temperature, may be thus marked, viz.

$$p,\ \varrho,\ \tau + i + \theta.$$

2dly. A portion of the air being extracted, and the pressure and density having decreased;

$$p - \delta p,\ \varrho - \delta\varrho,\ \tau + i + \theta.$$

3rdly. The communication with the external air being opened, and shut as soon as the pressure was observed equal to the first quantity p;

$$p,\ \varrho - \delta\varrho + \delta'\varrho,\ \tau + i + \theta + \delta' i,$$

$\delta'\varrho$ being the increase of density, and $\delta' i$ the latent heat disengaged by the condensation.

4thly. The heat of condensation $\delta' i$ being entirely dissipated, and the pressure having decreased a little,

$$p - \delta' p,\ \varrho - \delta\varrho + \delta'\varrho,\ \tau + i + \theta.$$

Now, at three of these epochs, namely, the 1st, 2d, and 4th, the temperature is the same; wherefore, the elasticities being as the densities, we get,

$$\frac{\delta p}{p} = \frac{\delta\varrho}{\varrho},$$

$$\frac{\delta' p}{p} = \frac{\delta\varrho - \delta'\varrho}{\varrho}.$$

Hence,
$$\frac{\delta\varrho - \delta'\varrho}{\delta\varrho} = \frac{\delta' p}{\delta p} = \frac{h - h''}{h - h'};$$

wherefore,
$$e = \frac{h - h''}{h'' - h'},$$

$$\delta\varrho - \delta'\varrho = e \times \delta'\varrho.$$

Again

Again, at the 1st and 3rd epochs, the elasticities being the same, the products of the densities by the factors for temperature will be equal: wherefore,

$$\frac{\delta\varrho - \delta'\varrho}{\varrho} = \frac{\alpha\delta' i}{1+\alpha\tau+\alpha i+\alpha\theta};$$

and by substituting the value of $\delta\varrho - \delta'\varrho$,

$$e \times \frac{\delta'\varrho}{\varrho} = \frac{\alpha\delta' i}{1+\alpha\tau+\alpha i+\alpha\theta}.$$

In this expression $\delta' i$ is the heat disengaged while the density acquires the increment $\delta'\varrho$. MM. Gay-Lussac and Welter have made a series of experiments between the temperatures -20^0 and 40^0 of the centigrade thermometer, and between the pressures $0^m{\cdot}144$ and $1^m{\cdot}46$; and they have found that the quantity e has in every case very nearly the same value. The equation is therefore generally true, at least within a great range. As the small variations may be considered as differentials, we obtain by integrating,

$$\varrho^e = C \times (1 + \alpha\tau + \alpha i + \alpha\theta),$$

and the determination of the constant is equivalent to making the original temperature equal to $\tau + \theta$, instead of τ. Wherefore, putting $\theta = 0$, we get

$$\varrho^e = \frac{1+\alpha\tau+\alpha i}{1+\alpha\tau},$$

the original density being unit, and the first temperature τ.

In the particular experiment of MM. Clement and Desormes, the numerical quantities are as follows,

$$h - h'' = 0^m{\cdot}00361$$
$$h'' - h' = 0^m{\cdot}01021$$
$$e = 0{\cdot}3492.$$

By another similar experiment of MM. Gay-Lussac and Welter, the value of e comes out equal to 0·37244. In both these determinations e is very nearly $\frac{1}{3}$; and it is not improbable, as I shall show below, that this is the true value. Adopting it, for the present, as an approximation, we get

$$\varrho = \left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)^3. \qquad \text{(B)}$$

In the experiments from which we have deduced the last formula, the only cause of a variation of temperature is the condensation and dilatation of the air. But in the manner we have viewed the subject, it is proved that ϱ is always the same function of the heat of combination, whatever be the sources of temperature. It follows, therefore, that the form of the function ϕ is determined generally by the formula (B). Hence we obtain the following expressions for the temperature,

ture, the density, and the elastic force of air in any proposed circumstances, viz.

$$p = \left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)^3 \times \frac{1+\alpha\tau+\alpha i+\alpha\theta}{1+\alpha\tau},$$
$$\varrho = \left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)^3, \qquad \text{(C)}$$
$$T = \tau + i + \theta.$$

If we suppose that air changes its volume while it retains the whole of its absolute heat, without receiving any addition of temperature from other bodies, the elastic force and density will be obtained by making $\theta = 0$ in the last equations:

$$p = \left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)^4$$
$$\varrho = \left(\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau}\right)^3 \qquad \text{(D)}$$
$$p = \varrho^{\frac{4}{3}}.$$

The last of these equations has already been published by M. Poisson in an article on the velocity of sound printed in the *Conn. des Tems* 1826.

3. Let V' denote the original volume of the air when the density is unit, and V its volume when the density is ϱ: then,

$$\varrho = \frac{V'}{V};$$

and by substituting this value in the equations (D), we get

$$\frac{1+\alpha\tau+\alpha i}{1+\alpha\tau} = \left(\frac{V'}{V}\right)^{\frac{1}{3}}, \qquad \text{(E)}$$
$$\frac{1+\alpha\tau+\alpha i+\alpha\theta}{1+\alpha\tau} = \frac{V}{V'} \times p.$$

From the first of these equations we learn, that when air contracts or enlarges its dimensions, the heat disengaged or absorbed follows the proportion in which the linear distance of the particles is lessened or augmented; a property which may furnish the means of examining experimentally the truth of this theory. On the other hand, the second equation shows that, when the pressure is constant, the changes of temperature are proportional to the variations of volume, which is a point already established pretty extensively by experiment.

What has just been said makes it very probable that $\frac{1}{3}$ is the true number, and that the experimental quantities are different only accidentally. For, till the fact of the case can be ascertained so as to leave no room for doubt, it is much more reasonable to suppose that the heat of combination follows exactly the variation of the linear distance of the particles, than

that it has to the changes of volume some other unknown relation nearly equivalent.

We may likewise infer (at least with great probability) that the same equations which express the laws of the condensation and dilatation of air will apply generally to all the permanent gases. Every gas absorbs heat when it enlarges its dimensions, and evolves heat when it contracts. The volume, or the density, is therefore in every case a function of heat the of combination. But it is proved, by the experiments of Dalton and Gay-Lussac, that, when the pressure is the same, the volumes of air and all the gases vary in the same proportion by equal changes of temperature. The second of the equations (E) is therefore true of all the gases for every given pressure. But if two volumes of air and a gas, subjected to the same pressure, vary in the same manner when exposed to equal degrees of heat, and if there be no other cause of the alteration of the volumes than the heat of combination, we must conclude that the common cause operates by the same rule in both the cases; that is, we must suppose that the first of the equations (E) is true of the gas as well as of the air. At least, the equations, if we suppose them generally true, will agree with all the facts, as far as is known; and they may therefore be considered as containing a physical account of the condensation and rarefaction of air and the gases.

What has been said of the gases will apply to the vapours exhaled from fluids, so long as they follow the same laws of condensation and dilatation with the gases. But in the vapours there is a maximum density when the quantity of latent heat is the least possible compatible with the gaseous form. In this case, when a greater degree of cold, or a greater compressive force is applied, as there is no source whence the heat necessary to a condensation can be supplied, a portion of the vapour returns to a fluid state.

The equations (E) show that a thermometer of air, or a gas, under a constant pressure, will exactly measure the variations of temperature by the changes of its bulk. But in reading the indications of such an instrument, the air must be allowed to resume a permanent volume, in order that the heat of combination i may be dissipated. Judging by analogy, we may infer that a thermometer of mercury or any other fluid, or a solid thermometer, such as a bar of metal, will be an exact measurer of temperature only so long as the cohesive force of the particles remains nearly the same. For in these cases the cohesion is the antagonist force to the expansion of heat, as the pressure is in the case of the gases.

4. One important application of the foregoing theory is to determine

determine the velocity of sound in the atmosphere. Newton led the way in this research. But the part of the *Principia* which treats of the nature and velocity of the aërial pulses is confessedly obscure, and the soundness of the reasoning has been called in question by philosophers of the first rank. Without attempting the discussion of a point on which it is certainly very difficult to form an opinion entirely free from objections, all are agreed that the velocity of sound assigned by Newton is just and accurate if we admit the law of elasticity on which his investigation proceeds. His results has been confirmed by every philosopher who makes the elastic force vary in the same proportion with the density. Yet when we appeal to experience, a great discordance is found between the fact and the theory. The velocity of sound determined by experiment is greater by a sixth part of the whole than the quantity computed by Newton's formula. No admissible explanation of this great difference was found, till Laplace happily conjectured that the true account of it was to be sought for in the law of Boyle and Mariotte. That law is exact only when the temperature remains unchanged. But a series of aërial pulses is a succession of condensations and rarefactions, accompanied with the disengagement and absorption of heat, and consequently with an increase of the air's elasticity. It is true that the variations of temperature are equalized and brought to the common standard in a short moment of time by the transference of heat between the atmosphere and the air in motion. But a sensible time is required to produce this effect; and the elastic force is exerted instantaneously, and while the agitated air retains all its heat of combination. Therefore, in the investigation of the velocity of sound, we ought not to adopt the law of Boyle and Mariotte, which supposes that the agitated air has the same temperature with the atmosphere: we ought to employ the elastic force of air, which changes its volume while it retains the whole of its absolute heat. When this ingenious explanation of Laplace is attended to, the difference between the theory and observation disappears, or is reduced to minute quantities that may reasonably be ascribed to unavoidable errors.

Conceive a slender horizontal tube of an indefinite length containing air in a state of equilibrium; and let x, reckoned from a fixed point in the axis of the tube, be the distance of a small cylinder of air within the tube, the thickness of which is equal to dx. Suppose now that the cylinder is pushed forward by some force to the distance $x + z$ from the fixed point, and that it occupies the length $dx + dz$ in the axis. It is to be observed that dx is invariably of the same magnitude,

whatever be the position of the small cylinder of air, and that dz alone varies in different places of the tube, and at different times. It follows therefore that x is independent on the time t, and z is a function of x and t. It is to be observed too that the air is supposed to undergo very small condensations and rarefactions in proportion to its original bulk in the state of equilibrium; that is, dz must be considered as very small when compared to dx. Let ϱ' denote the density of the air *in equilibrio*, and ϱ the variable density of the agitated cylinder; then, the masses of the two cylinders being the same, their densities will be reciprocally as the volumes: therefore

$$\frac{\varrho}{\varrho'} = \frac{dx}{dx+dz} = 1 - \frac{dz}{dx},$$

the powers of the small fraction $\frac{dz}{dx}$ being rejected. This equation, it may be remarked, implies the continuity of the fluid, since the cylinder in motion has always the same mass. Let P' denote the elastic force of the air *in equilibrio*, and P the like force of the agitated cylinder; then, if we adopt the law of Boyle and Mariotte, we shall have $\frac{P}{P'} = \frac{\varrho}{\varrho'}$: and this equation would lead us to the result obtained by Newton. But if, according to the observation of Laplace, we reason more agreeably to what actually takes place in nature, and suppose that the elastic force of the agitated cylinder is exerted while it retains the whole of its absolute heat, the preceding formulæ (D) will furnish this equation,

$$\frac{P}{P'} = \left(\frac{\varrho}{\varrho'}\right)^{\frac{4}{3}} = \left(1 - \frac{dz}{dx}\right)^{\frac{4}{3}} = 1 - \frac{4}{3}\cdot\frac{dz}{dx}.$$

Take the fluxions making x only variable, and divide by the equal quantities $\varrho(dx + dz)$ and $\varrho' dx$; then

$$\frac{dP}{\varrho(dx+dz)} = -\frac{4}{3}\cdot\frac{P'}{\varrho'}\cdot\frac{ddz}{dx^2}.$$

Now, P is the elastic force of the air in the tube at the distance $x + z$ from the assumed point in the axis, and $P + dP$ is the like force of the air at the distance $x + z + dx + dz$; wherefore dP is the effective force urging the intervening cylinder towards the assumed point: and as the mass moved is equal to $\varrho(dx + dz)$, the quotient is the acceleration of each particle, otherwise expressed by $-\frac{ddz}{dt^2}$;

wherefore $$\frac{ddz}{dt^2} = \frac{4}{3}\cdot\frac{P'}{\varrho'}\cdot\frac{ddz}{dx^2}.$$

Let l be the length of the homogeneous atmosphere, that is, of a column of air equal in weight to P' and having the same uniform

uniform density ϱ', then $\frac{P'}{\varrho' \times 1} = \frac{l \times \varrho'}{\varrho' \times 1} = \frac{l}{1}$; which shows that P' will impress the same velocity upon every particle of the mass $\varrho' \times 1$, that a row of aërial particles in the length l will do upon a single particle. Let $g = 32\frac{1}{6}$ feet denote the acceleration of gravity in a second of time: then since the weight of one particle will generate the velocity g in a second of time, the weight of l particles will generate the velocity $l \times g$ in the same time. Hence,

$$c^2 = \tfrac{4}{3} \times l g,$$

$$\frac{d\,d\,z}{d\,t^2} = c^2 \frac{d\,d\,z}{d\,x^2}.$$

This is the usual differential equation for determining the vibrations of a line of air; and the known integral proves that $c = \sqrt{\tfrac{4}{3} \times l \times g}$ is the expression of the velocity of sound in a second of time.

If, according to the law of Boyle and Mariotte, we had used the equation $\frac{P}{P'} = \frac{\varrho}{\varrho'}$, we should have found $c = \sqrt{g \times l}$ for the motion of sound in a second; and this is Newton's formula.

At the temperature of 32° of Fahrenheit, the homogeneous atmosphere is 4350 fathoms, or 26100 feet: and hence the motion of sound in a second is

$$\sqrt{\tfrac{4}{3} \times 32\tfrac{1}{6} \times 26100} = 1058 \text{ feet.}$$

Now the French Board of Longitude have lately found, by actual experiment, the velocity of sound at the same temperature equal to $331^m = 1085$ feet. The difference between the theory and observation is therefore only 27 feet, or about $\frac{1}{40}$ of the whole.

If, instead of making $e = \frac{1}{3}$, we adopt the values found by experiment, we shall have to employ the multipliers 1·3492 and 1·3724, instead of $\frac{4}{3} = 1{\cdot}3333$. By this means the calculation will approach a little nearer to the experimental quantity, although still short of it. But we should probably deceive ourselves by estimating with too much precision the result of experiments which require the measurement of very minute variations of length with extreme accuracy.

July 4, 1825. JAMES IVORY.

II. *Obser-*

II. *Observations on the Notes of Birds, including an Inquiry whether or not they are instinctive. By Mr.* John Blackwall.*

IT is much to be regretted that the study of ornithology is too frequently confined solely to the perusal of the best authors on the subject, and to the examination and arrangement of preserved specimens, whose faded plumage and distorted forms convey very imperfect ideas of the elegance and symmetry that so eminently distinguish this beautiful and highly interesting part of the creation. To those whom business or inclination leads to reside chiefly in large towns, such are almost the only means of information that offer themselves; but who that enjoys the opportunity of observing the free denizens of the fields and woods in their native haunts, would exchange their lively and unrestrained activity, their curious domestic economy, their mysterious migrations, and their wild but delightful melody, for the fixed glassy eye and the mute tongue of the inanimate forms that are crowded together in melancholy groups in the museum? Let me not, however, be misunderstood: I do not mean to insinuate, that those collections of birds that enrich the cabinets of the curious are of small utility; on the contrary, I am willing to allow that their importance is very considerable: but I would anxiously guard against an exclusive attention to the collecting and arranging of specimens, to the neglect of what is much more instructive and valuable; I mean the study of their habits, manners, economy, instincts, and notes. In these important particulars the history of birds is still very defective: the majority of authors, foreign as well as native, having limited themselves to the simple enumeration of specific characteristics and distinctions, and the occasional introduction of a few anecdotes, which from frequent repetition have, in general, lost much of the novelty they once possessed. We must except from this remark, however, the excellent works, in natural history, of our ingenious countryman the late Rev. Gilbert White, of Selborne in Hampshire, which abound with new and interesting facts. This diligent observer, whose example in investigating nature cannot be too highly recommended, instead of confining himself to the mere classification of natural objects, ranged the extensive wood, the tangled brake, the solitary sheep-walk, and the treacherous morass, to contemplate the manner of life, dispositions, and peculiar characters, of their fathered in-

* From the Memoirs of the Literary and Philosophical Society of Manchester, vol. iv.

habitants,

habitants, in their most sequestered retreats; and his writings bear ample testimony how well his researches were repaid. The subject, however, is still far from being exhausted: knowledge is acquired slowly; and even the most careful and indefatigable inquirers are liable to errors and omissions. Much yet remains to be supplied, much to be corrected, before the history of British birds can be pronounced complete.

To the practical ornithologist, who is desirous of promoting and extending his favourite study by the communication of his own personal observations and remarks, an intimate acquaintance with the various notes of the feathered tribes is of such vast importance, that any difficulties he may encounter in obtaining it, will be more than compensated by the numerous advantages it affords. In many instances it enables him to detect species that might otherwise elude his observation. Thus, the landrail, concealed in the long grass of luxuriant meadows, where it runs with great rapidity, and is sprung with difficulty; the grasshopper warbler, closely embowered in thick hedges and bushy dingles, where it employs every artifice to escape notice; and the sedge warbler, secluded amid the reeds and other aquatic productions of pools and marshes; are much more frequently heard than seen;—the harsh call of the first, the sibilous note of the second, and the hurried song of the last, being repeated through the night, in fine weather, during the breeding season.

It also enables him to identify species with the utmost precision: in some cases, indeed, with much greater certainty than he could by the minutest examination of good specimens. The three species of willow wren, for example, so strongly resemble each other, that even nice observers might have some difficulty in determining them by inspection; and, accordingly, we find that they have been the source of much confusion, perplexity, and error, among writers on ornithology. As their notes, however, are perfectly distinct, a little attention to them is sufficient to remove every difficulty. In the same manner, the crow may readily be distinguished from the rook, the raven from both, and the males of most species from the females.

The arrival of many of the periodical warblers is frequently first announced by their songs; and the clamorous night-calls of the redwing and fieldfare in the months of October and November, serve to establish the fact that these birds migrate, and that they perform their journeys in the dark.

But these are not the only advantages to be derived from an acquaintance with the notes of birds. As the feathered tribes communicate their sensations and intentions to one another

other through the medium of modulated sounds; the proficient, in what, without any impropriety, may be termed their language, can comprehend their various wants and emotions, and can participate in all their little joys and sorrows, hopes and fears: to him, the music of the groves is not a confusion of pleasing tones merely, but the melodious interchange of thought and feeling; which, though very limited and imperfect, still answers many important purposes, and contributes materially to the happiness and preservation of species. Thus birds that congregate and that live in society have usually a regular watch stationed in some commanding situation, whose note of alarm is understood by the whole community. Of the truth of this observation, fieldfares and rooks furnish familiar and striking instances. The shrill call of the swallow, the harsh scream of the jay, the petulant cries of the various species of titmouse, and the plaintive wailing of the flycatcher, likewise intimate the approach of an enemy. The reiterated cackle of the domestic hen after she has laid, speedily announces the joyful event; her cluck indicates that she has become the mother of a family; by a peculiar call she informs her brood whenever she discovers any thing suitable for food; and her shriek is a warning against impending danger. What is usually called the prating of poultry is expressive of satisfaction and complacency. But it is needless to multiply examples, or to insist further on the many useful purposes to which a familiarity with the language of birds may be rendered subservient: it will suffice to remark, that this knowledge supplies the means of making fresh discoveries, of correcting numerous errors, and of removing many of those doubts and difficulties that have arisen from the great similarity of some species, and the peculiarities incidental to age, sex, and a change of food or climate in others, without placing the observer under the painful necessity of destroying life,—a recommendation which will be duly appreciated by every one possessed of a humane disposition and a reflecting mind.

Having endeavoured in these few preliminary observations to point out the great importance of attending to the notes of birds, I shall now proceed to an inquiry into their origin,—an inquiry well calculated to exercise the skill of the experimentalist, and the ingenuity of the speculative philosopher, though to the generality of mankind it may seem trivial and of little moment.

The only author that I am acquainted with, who has treated this curious subject at any length, is the Honourable Daines Barrington. In an essay entitled Experiments and Observations on the Singing of Birds, published in the second part of the

sixty-

sixty-third volume of the Transactions of the Royal Society. And as the experiments there detailed appear to be imperfect and unsatisfactory, and the conclusions drawn from them hasty, unwarranted, and contrary to common experience; and, more especially, as this author is generally referred to by our cyclopædists*, and as his opinions seem to be finding their way into modern works of respectability, where they are quoted as established facts that do not admit of a doubt†; it was thought that an examination of his method of investigation would be useful in exposing its insufficiency, and the consequent looseness of the arguments founded upon it; while the institution of a less exceptionable course of experiments, it was hoped, might dissipate much of the obscurity in which this intricate question is at present involved. In what degree these expectations have been realized remains to be shown.

Mr. Barrington informs us, that his experiments were principally made with young linnets which were fledged, and nearly able to leave the nest; and the reasons assigned for this selection are, that birds of this species are docile, and possess great powers of imitation, and that the cocks are easily distinguished from the hens at an early period. These nestling linnets were educated under singing birds of various kinds; and it appears, that instead of the linnet's notes, they learned those of their respective instructors, to which they adhered almost entirely. In some instances, to be sure, the nestlings retained the call of their own species; which, as they were three weeks old when taken from the nest, it is supposed they had learned from their parents; and not unfrequently, when they had opportunities of hearing several species, they borrowed from more than one, and their songs became mixed‡.

To be certain that nestlings will not have even the calls of their species, Mr. Barrington remarks, that they should be taken when only a few days old. He then proceeds to notice instances of a linnet and a goldfinch taken at this early period, that came under his observation; acknowledging, at the same time, his own inability to rear birds of so tender an age. The

* See the Encyclopædia Britannica, art. Singing; and Rees's Cyclopædia, art. Song. † See Bingley's Animal Biography, vol. ii. p. 166—7.

‡ The reason given by Mr. Barrington for the steady adherence of birds in a wild state to their own songs, is, that they attend to the instructions of the parent birds only, disregarding the notes of all others. That young birds receive instructions in singing from the old ones appears to be a notion of great antiquity. Vide Aristot. *Histor. Animal.* lib. iv. cap. ix.—Plinii *Histor. Natural.* lib. x. cap. xxix. The celebrated Count Buffon seems to have entertained a similar opinion. See his *Histoire Naturelle des Oiseaux.* Tome cinquième, p. 47. Darwin also, in his Zoonomia, vol. i. p. 155, lends it the sanction of his authority.

first, he states, "belonged to Mr. Matthews, an apothecary at Kensington, which, from a want of other sounds to imitate, almost articulated the words 'pretty boy,' as well as some other short sentences;" and the owner assured him that it had neither the note nor call of any bird whatsoever. The goldfinch had acquired the song of the wren, without appearing to have a note or even the call of the goldfinch.

From these experiments and observations, of which I have given a concise, but I trust impartial account, Mr. Barrington was led to conclude, that "notes in birds are no more innate than language is in man, but depend entirely upon the master under which they are bred, as far as their organs will enable them to imitate the sounds which they have frequent opportunities of hearing." I am not aware, however, that he has brought forward a single fact, from which such an inference can be fairly deduced. The main tendency of his researches is merely to prove (what was before perfectly well known) that some birds have very extraordinary powers of imitation, and may be taught, when young, to sing the notes of other species, whistle tunes, or even pronounce a few words. If his remarks on this subject contain any novelty, it is, that birds so educated sometimes remain satisfied with these imitations, never blending any of their own notes with them; and, indeed, on this solitary circumstance, slight and inconclusive as it is, the entire weight of his arguments is rested. The instances of the goldfinch acquiring the song of the wren, and Mr. Matthews's linnet learning to articulate one or two short sentences, without having even the calls of their species, which this author seems to think so decisive, prove no more than his own experiments; which, as they were made, for the most part, with birds remarkable for their imitative powers, were certainly by no means well adapted to his purpose. As for the goldfinch, Mr. Barrington heard it only once, and then but for a short time; and that no dependence could be placed on any report of the people to whom it belonged, is evident from their supposing that it sang its own notes. These are circumstances that powerfully tend to invalidate almost every thing of importance that has been advanced respecting this bird.

In order to ascertain whether nestlings when taken very young will or will not have the calls and songs of their species, they should be kept in situations where they have no opportunity of learning any sounds that they may substitute for them; but this, I believe, has never yet been attempted.

I have already asserted, that Mr. Barrington's conclusions are contrary to common experience. I shall now endeavour to establish this charge.

It is well known to most persons who have the care and management of poultry, that ducks, guinea fowls, &c., hatched under the domestic hen, and domestic fowls hatched under turkeys, have the calls and habits peculiar to their species: that this is the case also with pheasants and partridges, brought up under similar circumstances, I have had frequent opportunities of observing. It is a matter of universal notoriety likewise, that all cuckoos of the species *Canorus*, though hatched and reared by birds of various descriptions, have constantly their proper calls *. These facts one would suppose were quite sufficient to convince the most prejudiced, that birds do not always acquire the calls and notes of those under which they are bred. But, perhaps, it may be urged, that ducks, guinea fowls, pheasants, and partridges, are probably incapable of learning the calls of domestic fowls; that domestic fowls, in their turn, may be incapable of acquiring the call of the turkey; and that the cuckoo appears to be very poorly qualified for imitating the notes of its foster parents. Still I must contend, that the incapacity of these birds has never been proved; and even if it had, it would afford no explanation of the manner in which they become acquainted with their own respective calls. According to Mr. Barrington's theory they ought to be mute; or, at least, should have such notes only as they have been able to pick up casually; which, of course, would possess little or no resemblance.

From these and similar observations, I have long been thoroughly convinced myself, that the calls of birds, which seem to be the simplest expressions of their sensations, are natural, not acquired; and in order to determine whether this is the case with their songs also, which are generally much more complex, and, consequently, have the appearance of being more artificial, the following experiments were made.

In the summer of the present year, (1822,) I procured three

* Mr. Barrington will not allow that the well-known cry of the cuckoo is a song, because it does not happen to accord with the conditions of his arbitrary definition; though, to the bird, it answers every purpose of a song, as well as the more elaborate effusions of the nightingale and skylark. Mr. Barrington defines a bird's 'song' to be a succession of three or more different notes, which are continued without interruption, during the same interval with a musical bar of four crotchets in an adagio movement, or whilst a pendulum swings four seconds; which necessarily excludes the chaffinch, redstart, hedge warbler, willow wren, and some others, that have always been accounted birds of song, as well as the cuckoo, from any pretensions to the title. Perhaps it would be more natural, and certainly less exclusive, to apply the term 'song' to those notes that are peculiar to the males; yet this definition would admit the peacock and turkey into the catalogue of singing birds; and the hideous scream of the one, and the ludicrous gobble of the other, are certainly any thing but musical.

young green grosbeaks,—a cock and two hens; which, as they did not see till the fourth day after they were taken from the nest, must then have been only two days old *.

These birds were reared by hand, in a house situated in the town of Manchester, where they had no opportunity of hearing the notes of any bird, except, perhaps, the occasional chirping of sparrows: nevertheless, they had all their appropriate calls, and the cock bird had the song peculiar to its species.

It was hoped, at the time, that this experiment would be considered sufficiently decisive; but recollecting that some persons, for the sake of showing their ingenuity in raising objections, might say that these birds remembered the notes of their parents, which they imitated as soon as they had acquired the power; and being willing to remove every circumstance on which the most fastidious inquirer could fix a doubt, I placed the eggs of a redbreast in the nest of a chaffinch, and removed the eggs of the chaffinch to that of the redbreast; conceiving that if I was fortunate in rearing the young, I should by this exchange insure an unexceptionable experiment, the result of which must be deemed perfectly conclusive by all parties. In process of time these eggs were hatched, and I had the satisfaction to find that the young birds had their appropriate chirps †.

When ten days old they were taken from their nests, and were brought up by hand, immediately under my own inspection, especial care being taken to remove them to a distance from whatever was likely to influence their notes. At this period, an unfortunate circumstance, which it is needless to relate, destroyed all these birds, except two,—a fine cock redbreast, and a hen chaffinch; which, at the expiration of twenty-one days from the time they were hatched, commenced the calls peculiar to their species. This was an important point gained, as it evidently proved that the calls of birds, at least, are innate; and that, at this early age, ten days are not sufficient to enable nestlings to acquire even the calls of those under which they are bred; thus clearly establishing the validity of the first experiment made with the young green grosbeaks. Shortly after, the redbreast began to record ‡, but in so low a tone, that it was scarcely possible to trace the rudiments of

* From numerous observations that I have made, it appears that young birds usually begin to see about the sixth day after they are hatched.

† Mr. Barrington defines the chirp to be the first sound a young bird utters as a cry for food. It consists of a single note, repeated at short intervals, and is common to nestlings of both sexes.

‡ The first endeavours of a young bird to sing are termed *recording*.

its

its future song in these early attempts: as it gained strength and confidence, however, its native notes became very apparent; and they continued to improve in tone till the termination of July, when it commenced moulting, which did not, as was expected, put a stop to its recording *. About the middle of August it was in deep moult, and by the beginning of October had acquired most of its new feathers. It now began to execute its song in a manner calculated to remove every doubt as to its being that of the redbreast, had any such previously existed †: its habits also were as decidedly characteristic as its notes; and I am the more particular in noticing this latter circumstance, because the peculiar habits of birds are quite as difficult to account for as the origin of their songs ‡. Thus it appears from this satisfactory experiment, which was conducted with the utmost care, that, contrary to Mr. Barrington's opinion, the notes of birds, which probably consist of those sounds that their vocal organs are best adapted to produce, are perfectly innate §.

Having shown that the notes of birds are natural, or, in other words, that they do not depend upon any previous instruction, it follows that they must furnish the attentive orni-

* The important operation of moulting undoubtedly affects the singing of wild birds very considerably; and may, perhaps, be a principal cause of their silence in the month of August. The London birdcatchers are well aware of the advantages of occasioning their call-birds to moult prematurely, which, by this means, are brought into full song, while other birds are nearly mute. For an account of the manner in which this is effected see Pennant's British Zoology, vol. ii. p. 332.

† Montagu, in the introduction to his Ornithological Dictionary, p. 29, states, in a note, that " a goldfinch, hatched and fostered by a chaffinch, retained its native notes," but does not give any further particulars respecting this bird.

‡ Several birds sing in the night, and some warble as they fly. The titlark uses particular notes in ascending and descending, and the song of the white-throat is accompanied with strange gesticulations. Larks and wagtails run; finches and buntings hop; nearly the whole of the gallinaceous and pie tribes, and many species of waterfowl walk; and woodpeckers climb. The sparrow, skylark, and most of the *gallinæ* are *pulveratrices;* and the kestril is, I believe, the only British hawk that hovers. The peculiar modes of flight and nidification are equally remarkable and worthy of notice; but, as they are foreign to the present subject, I shall not now dilate upon them.

§ Since writing the above, I have met with the following general assertion, unaccompanied by any evidence in support of it, in the Physiognomical System of Drs. Gall and Spurzheim; by J. G. Spurzheim, M.D. second edition, p. 194—5. " Singing birds, moreover, which have been hatched by strange females, sing naturally, and without any instruction, the song of their species as soon as their internal organization is active. Hence the males of every species preserve their natural song, though they have been brought up in the society of individuals of a different kind."

thologist

thologist with an excellent method of distinguishing species, under all the various circumstances that are liable to affect their plumage; though it must be observed, that the great similarity so evident in the songs of birds of the same species is more in tone and style, than in the individual notes of which they are composed *.

I shall here remark, that it is highly probable that no bird, in a wild state, ever borrows the notes of others, or becomes a mocker. I am well aware that several of our native birds (as the pettychaps and sedge warbler) have usually been termed mocking birds; but this is certainly improper: as they constantly use their own natural notes, and no others, they do not at all merit this appellation. The fine strain of the first has been thought to bear a striking resemblance to those of the swallow and blackbird: this, however, must be entirely imaginary, as it is totally different from them in manner and notes: if it be possible to trace any similarity between them, it will be found to consist in tone merely. The song of the sedge warbler is wonderfully varied, and appears to be chiefly composed of passages borrowed from the songs of the skylark, titlark, white-throat, whinchat, lesser redpole, swallow, &c. Now if any bird is entitled to the epithet of mocker, surely it is this: yet these resemblances are common to the songs of the whole species, which inhabit situations very unsuitable for acquiring some of them. In short, these fancied imitations are not studied, but purely accidental, consisting of their own notes *ab origine*.

The singing of birds has been very generally attributed to the passion of love, and a desire of pleasing their mates.

> " 'Tis love creates their melody, and all
> This waste of music is the voice of love;
> That even to birds and beasts the tender arts
> Of pleasing teaches †."

Thus the great poet of nature elegantly expresses the idea. This opinion, however, does not appear to be well founded: their language of love, their amorous strains, consist of low intermitted tones, accompanied with ridiculous gesticulations; and are altogether different from their ordinary songs, which seem to be occasioned by an exuberance of animal spirits, arising from an abundance of nourishing food, and an increase of temperature, and by a spirit of emulation and rivalry among the males. In confirmation of what is here advanced, I shall

* Birds of the same species do not always deliver their notes exactly in the same order of succession; neither do they uniformly use precisely the same notes.

† Thomson's Seasons,—Spring.

observe,

observe, that I have known many instances of birds having nests after they have entirely ceased singing; and that some species (as the woodlark, redbreast and wren) sing long after they have done breeding: caged birds also continue in song much longer than birds at large, though they have no mates to solace and amuse; and it is remarkable that almost any kind of continued noise is sufficient to stimulate them to sing. That birds of the same species distinguish each other by their notes, better than by any other circumstance, and that the songs of the males serve to direct the females where to seek their society, as Montagu has suggested, appears to me highly probable; but I must differ from this ingenious writer when he asserts that love is the sole cause of their songs*. In support of this opinion he states, that the males of our warblers, before they pair in spring, sing almost incessantly, and with great vehemence; that from the time of pairing till the hens begin to sit they are neither so vociferous, nor so frequently heard as before; that during the time of incubation their songs are again loud, but not so reiterated as at the first; and that so soon as the young are excluded from the eggs they cease singing entirely†: but it may be remarked, that if they are not heard so frequently and earnestly after pairing as before, most probably it is because they are occupied in attending to the females; and I have already observed, that their amatory notes, which they chiefly use at this period, are totally different from their ordinary songs. When the hens are sitting, or by any accident happen to be separated from their mates, the attention of the latter is much less engrossed; their notes of love are suspended, and their customary strains renewed. It is a very mistaken notion of Montagu, that the songs of these birds cease immediately when their eggs are hatched, as, in numerous instances, it is notorious that they continue even for some time after the young have left the nest. Surely it is needless to insist, that it cannot be love that prompts the young males to attempt their songs so soon as they are known to do‡: besides, it has been shown, that when educated early under other species, they sometimes possess their notes exclusively, which would hardly be the case if love is their only motive for singing.

For the information of those who may wish to be acquainted with the singing birds of this particular neighbourhood, I subjoin the following catalogue.

* This he does, in effect, in the introduction to his Ornithological Dictionary, p. 28, and following.

† See the introduction to the Ornithological Dictionary, pp. 30, 31.

‡ Young birds frequently begin to practise their songs when only a month old.

A Cata-

A Catalogue of Singing Birds, heard in the Neighbourhood of Manchester; with the Periods at which they commence and discontinue their Songs, taken at a mean of five Years' Observations.

Birds.	Commence Singing.	Cease Singing.
1. Redbreast, *Motacilla rubicula*	Jan. 3	Dec. 14
2. Wren, *Motacilla troglodytes**	do. 13	do. 3
3. Missel Thrush, *Turdus viscivorus*† .	Feb. 1	May 28
4. Throstle, *Turdus musicus*	do. 8	Aug. 12
5. Skylark, *Alauda arvensis*	do. 9	July 8
6. Hedge Warbler, *Motacilla modularis*	do. 9	do. 19
7. Chaffinch, *Fringilla cœlebs*	do. 10	do. 7
8. Starling, *Sturnus vulgaris*	do. 15	May 30
9. Blackbird, *Turdus merula*	Mar. 20	July 13
10. Green Grosbeak, *Loxia chloris* . . .	do. 24	Aug. 12
11. Titlark, *Alauda pratensis*	April 4	July 9
12. Lesser Redpole, *Fringilla linaria* .	do. 5	Aug. 5
13. Woodlark, *Alauda arborea*	do.	Oct. 25
14. Goldfinch, *Fringilla carduelis*	do. 11	June
15. Redstart, *Motacilla pnœnicurus* . . .	do. 14	do. 29
16. Willow Wren, *Motacilla trochilus* .	do. 14	Aug. 23
17. Linnet, *Fringilla linota*	do. 15	July 6
18. Lesser Fieldlark, *Alauda minor* . . .	do. 17	do. 8
19. Swallow, *Hirundo rustica*	do. 19	Sep. 25
20. Stonechat, *Motacilla rubicula*	do. 24	June
21. Whinchat, *Motacilla rubetra*	do. 25	July 1
22. Black-cap, *Motacilla atricapilla* . .	do. 25	do. 22
23. White-throat, *Motacilla sylvia* . . .	do. 29	do. 16
24. Pettychaps, *Motacilla hortensis* . . .	May 12	do. 11
25. Sedge Warbler, *Motacilla salicaria*‡	do. 17	do. 16

It would be difficult, nay impossible, to convey a distinct idea of the songs of these birds by any verbal description: indeed, the delightful associations they excite, with the adventitious circumstances of time, distance, situation, &c., so greatly

* The redbreast and wren sing at all times of the year, except during severe frost; and several species of birds that cease singing about the latter end of July, or the beginning of August, are sometimes heard again in autumn, when their songs are generally feeble, imperfect, and of short continuance, like the early efforts of our warblers in spring.

† The missel thrush is the largest British bird of song.

‡ In this catalogue I have omitted the yellow-bunting, reed-bunting, golden-crested wren, yellow willow wren, and some others, that have not uniformly been accounted singing birds.

influence

influence their effect, that even the best imitations are utterly inadequate to produce any thing equal to it.

Mr. Barrington, in his Essay, has attempted to construct a table, by which the comparative merits of British singing birds may be examined; but as he does not appear to have formed a correct estimate of the songs of some species, and as his table is inaccurate in other respects, besides being too limited, I have endeavoured to supply one that will be more comprehensive, and, I trust, less objectionable; making, as he has done, the number 20 the point of absolute perfection.

Birds.	Mellowness.	Sprightliness.	Plaintiveness.	Compass.	Execution.
1. Nightingale	19	14	19	19	19
2. Skylark	4	19	4	18	18
3. Black-cap	14	12	12	10	8
4. Pettychaps	14	6	14	10	9
5. Redbreast	9	8	12	14	14
6. Linnet	10	15	6	12	13
7. Woodlark	18	2	17	8	6
8. Goldfinch	4	16	4	10	12
9. Sedge Warbler* . .	2	16	0	18	14
10. Lesser Fieldlark . .	8	7	5	4	5
11. Willow Wren . . .	6	4	5	5	5
12. Throstle	3	10	2	10	4
13. Blackbird	8	1	4	5	3
14. Chaffinch	2	14	1	4	5
15. Green Grosbeak . .	5	3	5	5	5
16. Hedge Warbler . .	3	4	3	4	4
17. Wren	1	16	0	4	5
18. Swallow	4	6	2	3	3
19. Missel Thrush . . .	3	4	1	5	3
20. Starling	4	2	2	4	2
21. Titlark	3	2	2	2	2
22. Siskin	1	4	0	3	3
23. Lesser Redpole . . .	1	4	0	3	3
24. White-throat	1	4	0	3	3
25. Redstart	1	4	0	2	2
26. Stonechat	1	3	0	3	2
27. Whinchat	1	3	0	2	2
28. Dartford Warbler .					
29. Water Ouzel† . . .					

* Mr. Barrington has inserted the chaffinch, hedge-warbler, and reed-sparrow, in his table; which (according to his definition of a bird's song) ought not to have been admitted: indeed, the notes of the reed-sparrow

 are

This long catalogue of birds, most of which, it appears, are to be found in this immediate neighbourhood, composes the feathered choir that enlivens the pastoral scenery of England with a rich and varied melody of song, which probably is not surpassed in any part of the known globe.

The following poetical description of the vernal chorus, with which I shall close these observations, is from Thomson's Seasons,—Spring.

" Up springs the lark,
Shrill-voiced, and loud, the messenger of morn;
Ere yet the shadows fly, he mounted sings
Amid the dawning clouds, and from their haunts
Calls up the tuneful nations. Every copse
Deep-tangled, tree irregular, and bush
Bending with dewy moisture, o'er the heads
Of the coy quiristers that lodge within,
Are prodigal of harmony. The thrush
And wood-lark, o'er the kind contending throng
Superior heard, run through the sweetest length
Of notes; when listening Philomela deigns
To let them joy, and purposes in thought
Elate, to make her night excel their day.
The blackbird whistles from the thorny brake;
The mellow bullfinch answers from the grove:
Nor are the linnets, o'er the flowering furze
Pour'd out profusely, silent. Join'd to these,
Innumerous songsters, in the freshening shade
Of new-sprung leaves, their modulations mix
Mellifluous. The jay, the rook, the daw,
And each harsh pipe, discordant heard alone,
Aid the full concert; while the stock-dove breathes
A melancholy murmur through the whole."

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are so mean, that I am inclined to believe that he has attributed the song of the sedge warbler to this species, especially as he remarks, in a note, that it sings in the night, an error by no means uncommon among ornithologists,—yet, if this is the case, he has greatly underrated it; for though harsh in tone, and hurried in manner, and though the same note is repeated frequently in succession, it certainly possesses great variety, and is, upon the whole, rather agreeable.

† I have included the Dartford warbler, and the water ouzel, on the authority of Montagu. (See the Supplement to his Ornithological Dictionary.) The former I never saw alive, and therefore could have no means of estimating its song; and though I am well acquainted with the latter, I have never had an opportunity of hearing its notes.

III. Decas quarta novarum Plantarum Succulentarum; *Autore* A. H. Haworth, *Soc. Linn. Lond. —Soc. Horticult. Lond. —necnon Soc. Cæs. Nat. Cur. Mosc. Socio, &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

HEREWITH I transmit to you a fourth *Decade* of Succulent Plants, all rather recently received from the Cape of Good Hope, and by far the greater part of them first discovered there by the spirited and successful researches of Mr. Bowie; and all, save two, now flourishing in our gracious sovereign's collections at Kew, in the greatest perfection.

These plants are all interesting to the lovers of the Succulent tribes, and some of them are formed upon a plan as new as it is wonderful even to the eye of a Succulent botanist,—I allude more especially to the structure of the strange *Bulbine* hereunder enumerated and described. And they are all, as far as I have been able to discover, hitherto unrecorded in the books of botanical science. Trusting therefore that the communication will be acceptable to you, and find an admission into the next Number of your useful Journal,

I remain, sir, yours, &c.

Queen's Elm, Chelsea, July 6, 1825. A. H. Haworth.

Classis et Ordo. Pentandria Pentagynia.

Globulea *Nob. in Philosoph. Magaz. Sept.* 1824. *p.* 189, *&c.*

Sectio 1, Cultratæ.

radicans. G. (upright, rooting-branched) erecto-dumosa: ramis
1. confertis deorsùm radigeris, foliis lanceolato-ovatis cultratis; floribus arctè capitulatis.

Habitat C. B. S. ubi invenit Dom. Bowie. G. H. ♄.

Florebat in hyeme vernoque in nostro hortulo 1824-5, sed nondum in Horto Kewense. Amicus Dom. Aiton communicavit, A.D. 1823.

Obs. Suffrutex densè dumosus erectus pedalis (tertio anno) ramis fragilibus undique radiculas longas subramosas terram versus emittentibus fuscas, sæpe sesquiuncialis, sed terram apud nos non attingentes. *Folia* subuncialia sublanceolato-acinaciformia cultrata viridia mediocriter crassa integerrima horizontalia, internodiis longiora at satis distantia; suprà plana, subtùs convexiuscula, lentis ope supernè minutim at obsoletè punctulata. *Flores* in summis ramis terminales

minuti arctissimè geminatim capitulati sessiles *bracteis*que imbricatis densè suffulti, superioribus (bracteis) tenuioribus rotundioribus atque convexe-concavo-cochleariformibus. *Calyx* erectus, foliolis oblongis tenuibus convexis, intùs concavis imbricantibus corollam arctè adpressis pallidè viridibus, apice truncato-globosis albicantibus. *Corolla* inaperta albicans petalis imbricato-incurvis obtusè mucronatis concavo-cochleariformibus, infra apicem externè corpusculum globoso-claviforme vel obcordatum gerentibus. *Stamina* 5 corollâ breviora alba, *antheris* erectis latè oblongis utrinque sulcatis luteis. *Squamulæ* ordinariæ latè cuneato-truncatæ vel obsoletè obcordatæ aurantiæ. *Germina* 5, simul cum stigmatibus globulosis visa, omninò ampullæformia et ferè erecto-adpressa, staminibus breviora.

Obs. Caules florigeri s. pedunculi altiùs elongati sunt, sensim sensimque ac densiùs folioso-bracteolati.

Flores (fortè incompletos per hyemem) ferè perientes absque polline, sed certè ante anthesin, solùm examinavi.

Pone *Globuleam cultratam* locanda, at 3—4-plo minor longissimè ramosior et radicans: et distinguitur foliorum calycinorum obtusitate, florumque minoribus capitulis densioribus paucioribusque.

Sectio 2, Linguatæ.

lingua. 2. G. (great tongue-leaved) foliis elongatis, loratìm ventricosè semilanceolatis cultratis.

Habitat C. B. S. ubi invenit assiduus Bowie.

Florebat in Regio Horto Kewense Junio 1825. G. H. ♃.

Obs. Herba foliis imbricatìm decussatis dodrantalibus, unciam latis crassis impunctatis; et ad lentem ciliatis puberulisque, præcipuè in junioribus, et sæpè plùs minùs sericeo-canescentibus. *Scapus* paniculatus floribus numerosis densè confertis in capitulis hemisphæricis in summis paniculæ ramorum, affinium more et albicantibus. *Calyx* ordinarius foliolis per lentem ciliatis. *Corolla* ordinaria, calycem parùm superans petalis globulo extra apicem. *Stamina* alba cum *antheris* luteis corollâ inclusa. *Squamula* ordinaria cuneato-obcordata aurantia. *Germina* suboblongo-ovata, convexa intùs concava, stigmate capitato rotundo (absque stylo) finientia.

lingula. G. (lesser tongue-leaved) foliis subelongatis ventri-
3. cosè semilanceolatis cultratis tenuibus flaccidis.

Habitat C. B. S. ubi invenit assiduus Bowie. G. H. ♃.

Florebat in Regio Horto Kewense cum priore, Junio 1825.

Obs. Priori in omnibus simillima ut ovum ovo, at duplò plúsve minor, foliis tenuioribus s. minùs succulentis, flaccidis; florum capitulis minoribus, antheris stramineis, squamulâ ordinariâ fortè longiore.

Obs. This article and the preceding were announced (but not described) in the Phil. Mag. for September 1824 by the names above adopted.

Sectio 3, Loratæ. Acaules, foliis loratis impresso-punctatis, &c.

paniculata. G. (light-green impress-dotted) foliis lorato-acu-
4. minatis læte-viridibus, punctis minutissimis impressis: paniculæ ramis spicæformibus.

Habitat C. B. S. ubi invenit assiduus Bowie.

Florebat in Regio Horto Kewense Jul. 1825. G. H. ♃.

Obs. Acaulis lævis; foliis imbricatim decussatis 4-uncialibus sub-9 lineas latis, infrà convexis, suprà subconcavo-canalatis, margine minutè cartilagineo-albo ciliatis affinium more. *Scapus* validus erectus, ramis infimis 4-uncialibus, patulis teretibus crassitie pennæ columbinæ, bracteatis. *Bracteæ* distinctæ oppositæ, imæ distantes; superiores gradatim confertiores, ovato-acutæ amplexicaules quasi subfoliiformes, at redactæ. *Flores* in bractearum axillis, ferè confertim subsessiles, in singulâ axillâ subsex nidulantes. Infimæ flores bracteis humiliores, summæ bracteas superantes. *Calyx* 5-phyllus erectus corollâ duplò brevior, foliolis oblongis obtusis. *Corolla* nivea 5-partita ferè ad basin, *petalis* erectis oblongis, apice arctè incurvis extùs globulum parvum gerentibus. *Stamina* alba cum *antheris* luteis pollineque stramineo altitudine petalorum. *Squamula* ordinaria rhombea altè emarginata aurantia. *Germina* ovato-oblonga apice subrecurva obtusa, *stylis* nullis, neque *stigmate* conspicuo.

Obs. Pone G. *impressam* cui similis certè locanda, at lætè s. pallidè nec sordidè virens ut in illâ, punctis minoribus. Paniculæ flores non capitati, sed in ramulis spicatis thyrsove-spicatis sæpè 4-uncialibus et bracteatis ut in affinibus.

Sectio

Sectio 4, SUBULATÆ.

hispida. 5. G. (hispid flat-leaved) foliis confertis lorato-acuminatis subtùs convexis, cauleque tereti suffruticoso hispidis.

Habitat C. B. S. ubi invenit assiduus Bowie.

Florebat in Regio Horto Kewense Nov. 1824. G. H. ♄.

Obs. In sectione (dictâ) *Subulatæ*, nunc potiùs locum obtinet, (ante *G. mesembryanthoides* Nob.) quàm inter herbaceas quod tenebat in primâ vitâ*. *Caulis* florifer suffruticosus dodrantalis tertio anno, confertim ramulosus erectus, foliis contiguis viridibus patentibus biuncialibus, et ultrà, valdè undique hispidis seu hirsutis. *Flores* terminales glomerati inconspicui ut in affinibus; at incipientes in Octobris fine solùm vidi.

CRASSULA *Linn. &c.—Nob. in Philosoph. Magaz. Sept.* 1824, *p.* 186.

ericoides. 6. C. (large whipcord) erecto-decumbens: distantibus ramulis, foliis ovato-oblongis exiguis planis, quadrifariàm arctè imbricatis.

Habitat C. B. S. ubi invenit assiduus Bowie.

Florebat in Regio Horto Kewense Sept. 1823. G. H. ♄.

Descriptio. Suffrutex pulchellus et elegans, dodrantalis et ultrà, lævis ferè ericæformis floribusque Ericæ campanulatæ. *Caules* adultiores crassitie columbini calami quoque teretes. *Rami* primò erecti semipedales ramulis paucis alternis erectis et quasi densè articulatis, deinde debiliter decumbentibus pendulisve et radicantibus, rursúmque assurgentibus ramulis. *Folia* distincta (aëre aperto) eleganter ac densè imbricatim decussata expansa ovato-lanceolata pallidè viridia 3—4 lineas longa et internodiis 4—5-plò longiora, infrà convexula suprà plana, punctis (ad lentem) submarginalibus impressis obsoleto ordine. *Flores* (non eodem tempore aperti) terminales sub 5—10, in umbellulis cymosis laxis irregularibus foliosis, pedunculis subaxillaribus solitariis capillaribus,trilinearibus absque bracteolis. *Calyx* 5-phyllus foliolis teretibus grossis obtusis apice paululùm patulis, pedunculis brevioribus.

* This species was also announced, but not described, in the Philosophical Magazine for September 1824, and was, when young, supposed to belong to a stemless *Section*.

Corolla

Corolla parva campanulata nivea petalis 5 (ordinariis) basi solùm coalitis et imbricantibus, erectim incurvo-recurvulis inflexo-concavis ovato-oblongis calyce sesquilongioribus mucrone obtuso infra apicem externè. *Filamenta* alba. *Antheræ* erectæ oblongo-cordatæ obtusæ rubræ polline luteo. *Styli* 5 albi erecti adpressi (cum germinibus) obclavati, stigmatibus inconspicuis atque quàm antheris humilioribus aliquantillùm.

Obs. *Folia* juniora lævissimè connata, et basi (lente optimo) minutissimè albo-ciliata, ciliis decurrentulis paululùm. Emortua folia sæpè plùs minùs persistentia, erecta tortave emarcida.

Nulla mihi nota species affinis. A *Crass. imbricatâ*, quæ sessiles laterales flores gerit, differt, foliis duplò majoribus distantioribus, necnon terminalibus cymoso-umbellatis floribus: sed fortassè approximat Linnæi *Crassulam pyramidalem.*

Obs. In this place I avail myself of the opportunity of giving a further description of *Crassula rotundifolia* of page 188 of the Philosophical Magazine for September 1824, whose flowers I had not then dissected, and which prove it to be a genuine species of *Kalanchöe*, viz. Kalanchöe *rotundifolia:* subherbacea erecta, foliis petiolatis subrotundis paucidentatis imis integerrimis.

Descriptio. *Caulis* florifer pedalis et ultrà. *Flores* terminales corymboso-paniculati. *Pedunculi* tereto-clavati glabri (uti totâ plantâ) *Calyx* 4-phyllus foliolis subtriangularibus plano-turgidis acutis, et (cum corollâ) germine adpressis. *Corolla* 4-fida (in Januario mense) inaperta, pallidè rufa s. aurea, infernè pallidior, laciniis angustis acutis. *Stamina* 8, viridi-lutescentia tubo ferè ad apices connata, horum 4, ferè altitudine tubi, et 4 alternantia breviora. *Germina* erecta grossa viridia, supernè cum *stylis* brevissimis continuantibus attenuata, *stigmatibus* in lente, minutis rotundatis hyalinis, embryonibus (germinis) incipientibus numerosis rotundis pellucentibus.

A *Kalanchöe ægyptiacâ* (cui proxima) discrepat calyce adpresso, foliisque rotundioribus.

Classis et Ordo. Hexandria Monogynia.

Bulbine. *Willd. Enum.* 372.—*Nob. in Revis. Pl. Succ.* 32.

mesembryanthoides. B. (hemispherical-leaved) acaule: foliis 7. sub-

sub-tribus arctè imbricatim erectis truncatis pulposis subhemisphæricis.

Habitat C. B. S. ubi invenit assiduus Bowie.—G. H. ♃.

Florebat in Regio Horto Kewense Maio 1822, iterúmque 1823.

Obs. Planta verè mirabilis. Subsemiglobosam pulposam et semuncialem appareat, simulantem *Mesembryanthema* minima, at singula planta composita est circiter tria folia inæqualia (inferiora minora) imbricatim-arctissimè adpressa, sub hemisphærico-truncata pallidè viridia, apicem versus saturatiora et subaristata; internè concava. *Scapus* erectus 4—5-entalis filiformis nudus. *Flores* laxè spicati sub 3—4, alterni pedunculati lutei, at morientes solùm vidi. *Stylus* 1. *Capsula* obtusè sulcata sive angulata.

Classis et Ordo. Decandria Pentagynia.

Cotyledon, *Linn. &c.*

tricuspidata. C. (trident-leaved) farinoso-alba, foliis angustis
8. sæpè altè tricuspidatis.

Habitat C. B. S. G. H. ♄.

In hortis occurrit sub hoc nomine. *C. papillari* simillima, at satis differt ut in charactere suprà, folisique subinde trifidis. *Flores* adhuc non produxit.

Obs. In this place an opportunity is afforded of making a few additional and useful remarks respecting two species of this genus, which were first published in my *Suppl. Pl. Succ.* and in my *Revis. Pl. Succ.* from weak or incomplete examples: viz.

1. The stem of *C. elata* described in *Suppl. Pl. Succ.*, p. 20, was tied up and appeared firm; but my own plant has ever had a "caulis subdebilis, basi ramosus," and will probably so remain, although it is a radical offset from the plant I described; because gooseberry bushes, &c. raised from decumbent suckers are said never to make upright plants.

2. Cotyledon coruscans, *Suppl. Pl. Succ.* p. 21, was described from a young green plant in a very hot stove at Kew, glittering in the sun's beams very much; but grown in a greenhouse it glitters not at all, and usually becomes "farinoso-alba, ramosa, vix pedalis; foliis decussatis confertis incurvo-recurvis canaliformibus semipedalibus unciam latis crassis; apice tenuioribus planatis rotundatis, cum mucrone; *pedunculo* umbellato

bellato subpedali nudo, medio et supernè subbibracteolato, umbellâ repetito-trichotomâ, seu trifidâ corollis ferè ut in *Cot. orbiculari*, at pallidioribus." Wherefore the specific name of *coruscans*, being one that misleads, is here changed to that of

canalifolia. C. (channel-leaved farinose.)

rhombifolia. C. (rhomb-leaved farinose) farinoso-alba: foliis
9. approximatis obovato-rhombeis mucronatis: caule ramoso valido decumbente.

Habitat C. B. S. G. ♄.

Communicavit amicus Parmentier A.D. 1823.

Obs. Cotyledoni hemisphæricæ proxima at magìs ramosa, longè humilior, foliis minoribus supernè non ampliatis in arcum, sed rhombeis, magìsque acuminatis; margine à mèdio ad apicem (cum mucrone) eleganter (aëre aperto) purpurascentibus. *Flores* non vidi.

Classis et Ordo. Dodecandria Trigynia.

Euphorbia, *Linn. &c.*

grandidens. E. (rosy-spined triangular) aculeata erecta:
10. ramis simplicibus triquetris marginibus grandidentatis, dentibus bispinosis, spinis validis divaricantibus.

Habitat C. B. S. ubi invenit assiduus Bowie. G. H. ♄. Viget in Regio Horto Kewense. Erecta firma (nunc bipedalis), ætate teretiuscula, ramis simplicibus confertis in superiore párte plantæ, àt mox fortè deciduis. Aculei validi subdivaricantes lætè rosei (morientes nigri) subbilineares infra foliolos orientes. *Foliola* ut in affinibus exigua, plana subtriangularia sessilia viridia lineam longa.

Species insignis, locanda pone *E. lacteam.* Flores non vidi.

IV. *On the Declination of the principal Fixed Stars, as observed at Greenwich in the Year* 1822. *By Professor* Bessel*.

THE degree of perfection obtained by the observations at Greenwich, from the plan (adopted since 1821) of observing with the mural circle not only the stars themselves, but also their reflections from a horizontal surface, makes an

* From Schumacher's *Astronomische Nachrichten*, No. 73.

exact comparison of those results with my own so interesting, that I have long formed the desire of applying to Mr. Pond's original observations the same method of calculation which I applied to my own. Taking therefore advantage of the numerous observations (direct as well as reflected) contained in the volume of 1822, obligingly sent to me by Mr. Pond through the kind intervention of Dr. Tiarks, I requested M. Olufsen, who is now here, to assist me in their reduction. I now communicate the results of these labours to the lovers of astronomy, since the explanation which they give, respecting the difference between Mr. Pond's *standard* catalogue and mine, is calculated to establish the true state of the case.

M. Olufsen has reduced all the observations of the pole-star and fundamental stars made with the six microscopes, to the beginning of 1822, by means of the Königsberg table of refraction, and Professor Schumacher's Ephemerides.

The pole-star has been observed by direct vision, at 123 upper and 144 lower culminations; and by reflection, with 27 upper and 45 lower ones. Those direct observations, at both culminations, which could be advantageously combined for finding the declination of the pole-star, have given the correction of my tables, deduced from Mr. Pond's determinations in 1813, with the weight of 192 observations, $= -0'',35$; agreeing to within one hundredth of a second with my determination, in part vii. of my Observations, page xxiv. After having corrected the declinations contained in Schumacher's tables, by $-0'',35$, we calculated, from the direct observations of this star, the places of the pole upon the instrument; and then the mean, taken out of every ten successive observations, contained in the following table, with the corresponding days:

	°	′	″		°	′	″
Jan^{y}. 16	89	59	4·88	June 14	89	59	58·85
Feb^{y}. 2			5·62	24			58·39
16			5·84	July 15			57·54
24			6·45	Aug. 14			57·25
Mar. 12			6·41	Sept. 17			58·38
April 7			8·22	Oct. 6			58·59
23			9·58	20			57·65
May 3			7·57	29			56·42
16			7·76	Nov. 10			56·84
21			7·32	20			57·05
28			5·26	28			57·51
June 3			4·89	Dec. 7			58·19
8			5·00	18			57·96
				28			58·59

The

The result of this table is, that the apparatus (probably the pillar to which the microscopes are fastened) was not quite unchangeable; for which reason the places of the pole, for those days not contained in the table, have been determined by interpolation. By means of this table, 72 observations by reflection of the pole-star showed the distance of the reflected from the true pole $= 257^\circ\ 2'\ 43'',13$, after the reduction of the vessel containing the quicksilver (assumed at 5 feet 4 inches below the centre of the instrument) to the instrument. The results of the other stars are contained in the following table:

	Direct.				Reflected.			
	°	′	″		°	′	″	
α Aurigæ . .	44	11	41·72	64	212	51	1·76	11
α Cygni . . .	45	21	5·66	67	211	41	37·03	38
α Lyræ . . .	51	22	35·45	74	205	40	8·26	50
α Geminor. .	57	43	52·84	58	199	18	50·79	8
β ———	61	33	9·90	57	195	29	34·69	4
β Tauri . . .	61	33	10·74	46	195	29	31·53	5
α Andromed.	61	53	32·72	18	195	9	9·35	10
α Coronæ .	62	40	49·87	29	194	21	53·19	18
α Arietis . .	67	23	1·53	46	189	39	40·23	18
α Bootis . .	69	53	11·41	86	187	9	30·52	34
α Tauri . . .	73	51	26·03	61	183	11	15·88	8
β Leonis . .	74	25	59·93	16	182	36	44·03	17
α Herculis .	75	23	56·97	18	181	38	44·89	13
α Pegasi . .	75	45	1·03	27	181	17	40·25	20
γ ——— . .	75	48	22·30	9	181	14	19·57	8
α Leonis . .	77	9	58·77	48	179	52	42·99	15
α Ophiuchi.	77	18	8·90	21	179	44	34·72	12
α Aquilæ . .	81	35	40·05	61	175	27	2·66	43
α Orionis . .	82	38	6·32	52	174	24	34·80	6
α Serpentis .	83	0	27·45	34	174	2	16·64	15
β Aquilæ . .	84	1	50·61	9	———			—
α Canis min.	84	19	35·65	60	172	43	7·71	7
α Ceti . . .	86	36	51·28	35	———			—
α Aquarii. .	91	10	49·35	33	165	51	52·87	15
α Hydræ . .	97	53	30·84	15	159	9	12·28	9
β Orionis . .	98	24	53·81	11	———			—
α Virginis .	100	13	44·26	25	156	48	58·69	23
2α Capric. .	103	5	18·99	26	———			—
1α Libræ . .	105	15	1·97	8	151	47	39·36	7
2α ——— . .	105	17	45·02	4	151	44	57·71	2
α Canis maj.	106	28	45·90	59	150	33	58·42	7
α Scorpii . .	116	1	38·92	15	141	1	4·05	7
α Piscis austr.	120	33	45·49	8	———			—

Assuming

Assuming that the readings of the instrument require a correction for the effect of gravity upon it, of the form

$$a \sin z + b \cos z$$

in which z signifies the distance from the zenith (reckoning from 0 to 360° from the zenith to the south), we obtain from the measured distance P of a star from the pole, its declination

$$\delta = 90° - P - (a \cos \phi - b \sin \varphi) - a \sin (\varphi - \delta) - b \cos (\phi - \delta)$$

and from the reflected image,

$$\delta = P + 2\phi - 270° + a \cos \phi - b \sin \varphi + a \sin (\varphi - \delta) - b \cos (\phi - \delta)$$

The result of the combination of the two formulæ is

$$360° - 2\varphi = P + P' + 2 (a \cos \varphi - b \sin \varphi) + 2a \sin (\varphi - \delta)$$

Or, by putting the latitude of Greenwich,

$$\varphi = 51° 28' 39'' + y \quad \text{and} \quad P + P' = 257° 2' 42'' + m$$

we have $0 = m + 2 (y + a \cos \phi - b \sin \phi) + 2a \sin (\phi - \delta)$

$$= m + 2x + 2a \sin (\phi - \delta)$$

where x is written for $y + a \cos \varphi - b \sin \varphi$.

The following are the equations which result from the different stars:

Polaris . . 0 =	+ 1″·13	+ 2x	− 1·246 a
α Aurigæ	+ 1·48	+ 2x	+ 0·198 a
α Cygni	+ 0·69	+ 2x	+ 0·238 a
α Lyræ	+ 1·71	+ 2x	+ 0·445 a
α Geminorum . . .	+ 1·63	+ 2x	+ 0·658 a
β ——— . . .	+ 2·59	+ 2x	+ 0·782 a
β Tauri	+ 0·27	+ 2x	+ 0·782 a
α Andromedæ . .	+ 0·07	+ 2x	+ 0·793 a
α Coronæ	+ 1·06	+ 2x	+ 0·818 a
α Arietis	− 0·24	+ 2x	+ 0·965 a
α Bootis	− 0·07	+ 2x	+ 1·041 a
α Tauri	− 0·09	+ 2x	+ 1·157 a
β Leonis	+ 1·96	+ 2x	+ 1·173 a
α Herculis	− 0·14	+ 2x	+ 1·200 a
α Pegasi	− 0·72	+ 2x	+ 1·210 a
γ ——	− 0·13	+ 2x	+ 1·212 a
α Leonis	− 0·24	+ 2x	+ 1·249 a
α Ophiuchi	+ 1·62	+ 2x	+ 1·252 a
α Aquilæ	+ 0·71	+ 2x	+ 1·366 a
α Orionis	− 0·88	+ 2x	+ 1·392 a
α Serpentis	+ 2·09	+ 2x	+ 1·402 a
α Canis min. . . .	+ 1·36	+ 2x	+ 1·434 a
α Aquarii	+ 0·23	+ 2x	+ 1·590 a
α Hydræ	+ 1·12	+ 2x	+ 1·721 a
α Virginis	+ 0·95	+ 2x	+ 1·761 a
1α Libræ	− 0·67	+ 2x	+ 1·838 a
2α ——	+ 0·73	+ 2x	+ 1·838 a
α Canis maj. . . .	+ 2·32	+ 2x	+ 1·854 a
α Scorpii	+ 0·97	+ 2x	+ 1·953 a

Hence

Hence it follows that the value of a, in the Greenwich instrument, must be very small. Mr. Pond, indeed, considers it as vanishing altogether. However, by keeping entirely to the observations, and resolving the above 29 equations after the method of minimum squares, we shall find

$$a = +\,0'',239, \quad \text{and } x = -\,0'',5082$$

and hence the latitude is

$$51^\circ\,28'\,39'' + x - a\cos\varphi + b\sin\varphi = 51^\circ\,28'\,38'',3429 + 0'',786\,b$$

in which the dependence of the last term on b could have been removed only by reversing the instrument; a measure however which, by its construction, is rendered impracticable. In order that we may resolve this calculation effectually, I have first determined the probable error of the direct observations, as well as of those of the reflected ones. I find the same result for the former, out of 412, and for the latter out of 265 observations, viz. $\pm\,0'',799$. Whence it appears, that Mr. Pond must have secured the vessel containing the quicksilver from every kind of concussion, since the probable error of the observations by reflection has not been in the least increased. From this equality of value by both methods of observation, each of the above equations (resting upon a observations by direct vision, and a' observations by reflection) give the weight $a\,a' : (a + a')$. According to this weight I have made the calculations. The coefficient of the sine of the zenith-distance has received the weight of 366 observations.

According to the formulæ above mentioned the declination of a star is, from the direct observations,

$$= 90^\circ - \mathrm{P} - 0'',1489 - 0'',239\sin(\varphi - \delta) + b\,[\sin\varphi - \cos(\varphi - \delta)]$$

and from the observations by reflection

$$= \mathrm{P}' - 167^\circ\,2'\,42'',8653 + 0'',239\sin(\varphi - \delta) + b\,[\sin\varphi - \cos(\varphi - \delta)]$$

Hence it follows that the bend of the instrument, depending on b, can be as little eliminated as in the determination of the latitude: whence nothing remains but to neglect it altogether. For $\delta = 2\varphi - 90^\circ = +\,12^\circ\,57'$ it has no influence on the declination; for northern declination its coefficient is negative; for southern, positive. The declinations resulting from the direct observations and those by reflection, under the assumption of $b = 0$, as well as to the mean taken with respect to the number, is contained in the following table:

	Declination for 1822.			Most probable Declination for 1822.			
	Direct.			Reflect.			
	°	′	″	″	°	′	″	
α Aurigæ . .	+45	48	18·11	18·62	+45	48	18·19	75
α Cygni . . .	44	38	54·16	53·89	44	38	54·06	105
α Lyræ . . .	38	37	24·35	25·15	38	37	24·67	124
α Geminorum	32	16	6·93	7·70	32	16	7·02	66
β ———	28	26	49·86	51·62	28	26	49·97	61
β Tauri . . .	28	26	49·02	48·46	28	26	48·97	51
α Andromedæ	28	6	27·04	26·28	28	6	26·77	28
α Coronæ . .	27	19	9·88	10·12	27	19	9·97	47
α Arietis . .	22	36	58·20	57·18	22	36	57·91	64
α Bootis . . .	20	6	48·32	47·48	20	6	48·09	120
α Tauri . . .	16	8	33·68	32·85	16	8	33·58	69
β Leonis . .	15	33	59·78	61·01	15	34	0·41	33
α Herculis .	14	36	2·74	1·87	14	36	2·37	31
α Pegasi . .	14	14	58·68	57·23	14	14	58·06	47
γ —— . .	14	11	37·41	36·55	14	11	37·00	17
α Leonis . .	12	50	0·93	59·98	12	50	0·70	63
α Ophiuchi .	12	41	50·80	51·71	12	41	51·13	33
α Aquilæ . .	8	24	19·64	19·66	8	24	19·65	104
α Orionis . .	7	21	53·36	51·80	7	21	53·20	58
α Serpentis .	6	59	32·23	33·64	6	59	32·66	49
β Aquilæ . .	5	58	9·07	——	5	58	9·07	9
α Canis min.	5	40	24·03	24·72	5	40	24·10	67
α Ceti	3	23	8·39	——	3	23	8·39	35
α Aquarii . .	− 1	10	49·69	50·10	− 1	10	49·82	48
α Hydræ . .	7	53	31·20	30·68	7	53	31·01	24
β Orionis . .	8	24	54·17	——	8	24	54·17	11
α Virginis . .	10	13	44·62	44·26	10	13	44·45	48
2α Capricorni	13	5	19·36	——	13	5	19·36	26
1α Libræ . .	15	15	2·34	3·58	15	15	2·92	15
2α ——— . .	15	17	45·39	45·23	15	17	45·34	6
α Canis maj.	16	28	46·27	44·52	16	28	45·79	66
α Scorpii . .	26	1	39·30	38·88	26	1	39·17	22
α Piscis austr.	30	33	45·88	——-	30	33	45·88	8

The declinations contained in the last column can, on account of their having been calculated with the Königsberg table of refraction, be immediately compared with my catalogue. The following table shows their differences from my declinations, as well as the differences of Mr. Pond's *standard* catalogue and his list for 1822 (*Astron. Nachr.* No. 28), by which the changes which these differences have gradually undergone may be seen at once:

	Standard Catalogue.	Pond 1822.	New Calculation.
	"	"	"
α Aurigæ . .	+ 1·88	+ 0·49	+ 0·13
α Cygni . . .	+ 2·42	+ 1·47	+ 0·46
α Lyræ . . .	+ 2·39	+ 2·11	+ 0·97
α Geminorum	+ 2·05	+ 1·08	+ 0·36
β ———	+ 1·57	+ 1·10	+ 0·61
β Tauri . . .	+ 2·02	+ 1·44	+ 1·16
α Andromedæ	+ 3·15	+ 1·30	+ 0·27
α Coronæ . .	+ 2·71	+ 2·40	+ 0·49
α Arietis . . .	+ 2·69	+ 0·92	+ 0·89
α Bootis . . .	+ 2·45	+ 2·42	+ 0·67
α Tauri . . .	+ 2·54	+ 1·55	+ 0·72
β Leonis . . .	+ 2·09	+ 2·14	+ 0·54
α Herculis .	+ 3·18	+ 3·14	+ 1·14
α Pegasi . . .	+ 4·13	+ 1·59	+ 0·49
γ ——— . .	+ 2·98	———	+ 0·73
α Leonis . . .	+ 2·61	+ 2·87	+ 1·74
α Ophiuchi .	+ 3·27	+ 3·12	+ 1·71
α Aquilæ . .	+ 3·43	+ 2·93	+ 0·95
α Orionis . .	+ 3·60	+ 1·32	— 0·01
α Serpentis .	+ 3·24	+ 3·75	+ 1·40
β Aquilæ . .	+ 4·39	———	+ 1·25
α Canis min.	+ 4·22	+ 2·34	+ 1·26
α Ceti	+ 3·15	+ 1·90	+ 1·74
α Aquarii . .	+ 4·19	+ 1·97	+ 1·27
α Hydræ . .	+ 3·54	+ 2·82	+ 1·22
β Orionis . .	+ 3·15	+ 1·72	+ 0·74
α Virginis . .	+ 3·16	+ 3·95	+ 1·29
2α Capricorni	+ 5·35	———	+ 2·90
1α Libræ . .	+ 6·66	+ 4·82	+ 1·15
2α ——— . .	+ 4·65	———	+ 0·45
α Canis maj.	+ 5·16	+ 1·68	+ 0·33
α Scorpii . .	+ 5·74	+ 4·88	+ 1·12
α Piscis austr.	———	———	+ 5·13

On comparing this list, we find that the differences of the new calculation and the Königsberg catalogue are both smaller and more uniform, than the difference of the two Greenwich catalogues compared with one another. The reason of the greater regularity seems to lie partly in the more correct reductions which are, without any trouble, obtained by Schumacher's tables, and partly in the application of more accurate corrections of the refraction caused by the state of the thermometer;

meter; in which Bradley's rule perceptibly deviates from the truth. The regularity hereby obtained is of about the same magnitude as the probable errors would lead us to expect.—The reason of the greater deviation at 2α *Capricorni* is unknown to me; that of α *Piscis austr.* may arise from the centre of the long coloured spectrum (which this lowly culminating star presents) having been differently determined by two different observers.

The constant difference of both series of observations has been so greatly diminished by this calculation, that it is no longer so striking as before. But, whilst the casual errors of the single observations are reduced to a trifle, by the increase of their number, the goodness of the instruments, and the care in their use, the imperfections of the elements of reduction, hitherto unnoticed, distinctly appear. If, after the increased certainty of Mr. Pond's observations (by the application of the method of observing by reflection), the difference were still as great as in the *standard* catalogue, yet we should be led to believe that there are other unknown sources of error. Now, however, I think I am able to point out some circumstances which might account for differences of this magnitude.

If, for instance, we were to increase the logarithms of the Greenwich refractions by about 0·0035 (which for 45° of zenith distance amounts to $0''{,}47$), the differences would become partly positive, partly negative. Such a change would lead us to suppose that either the barometer stands 0·24 inches too low, or the thermometer 4° Fahrenheit too high. Or, distributing the error on both, that the first is 0·12 inch too low, and the second 2° too high. We might add that the sense in which both errors must be assumed, is agreeable to former experience: for the barometers often stand too low, owing to air having been introduced by long use, or to the bore of the tube not being corrected by the scale; respecting which I find no notice with reference to the barometer of Greenwich. The height of my barometer, for instance, would be 1 line = 0·09 inch (English) too short, if I were to use them without correction: the freezing point also of the thermometer moves upwards in time; although I do not think that changes of this kind, exceeding 1° Fahr., have ever been observed. If Mr. Pond would be so kind as to give some account of the condition of his meteorological instruments, we should be able to decide at once, whether any difference, and how much thereof, is to be ascribed to this cause. I might readily have determined the *constant* of refraction, from Mr. Pond's observations (which would have removed every doubt concerning the meteorological instruments) had there been among the lower culminations,

culminations, in the observations for 1822, any that had been fit for it. The volume for 1821 I have not yet seen; and to employ earlier ones I consider unadvisable, since it is probable that the instrument now gives quite different polar distances from what it used to do.

However, I am not of opinion that an imperfection in the meteorological instruments is absolutely necessary for the explanation of the difference between the two series of observations: on the contrary, I believe that this difference may be also explained from the supposition already made,—that the bend of the instruments is such as may be removed by the application of counterpoises invariable in all situations relative to the horizon. From this supposition follows the formula $a \sin z + b \cos z$. However, I have reason to think that this ground may be essentially erroneous, which I intend to explain in another place.

For the present, the question respecting the difference between the Greenwich and Königsberg observations seems to me to stand thus:

1°. That there can be no doubt but that the mural circle at Greenwich (probably by the strengthening of the telescope undertaken in 1821) has now given larger polar distances than before; as is proved by the catalogues of 1813 and 1822.

2°. That the difference has been so far reduced from the perceptible magnitude, which it had according to the *standard* catalogue (partly by Mr. Pond's own subsequent catalogue, partly by M. Olufsen's calculation of the observations of 1822, made after my refractions), that the remaining part may be readily explained from very probable causes.

BESSEL.

V. *On the Construction of the large Refracting Telescope just completed. By M.* FRAUNHOFER. *Read at a Meeting of the Royal Bavarian Academy of Sciences, the* 10*th of July* 1824 *.

THE instrument, of which I have the honour of speaking, is destined for the Imperial Observatory at Dorpat. It is the largest of its kind, and new in various parts of its construction.

The largest telescopes hitherto used were those constructed with metal mirrors. But since even the most perfect of these mirrors reflect but a small portion of the light it receives (the larger portion of it being absorbed), the mirror telescopes must

* From Schumacher's *Astron. Nachrichten*, Nos. 74 and 75.

be of an immense size to produce any effect; on which account the intensity of light that strikes the eye of the observer is still very small. Nor is it possible entirely to rectify the deviation of the rays occasioned by the spherical forms of the reflecting surfaces of these telescopes;—reasons which, with various others, have rendered the mirror telescopes of little value for mathematico-astronomical observations, and have caused their rejection as meridional instruments, &c.

The glass, on the other hand, allows all the rays to pass; and not only compensates in a telescope, made of flint- and crown-glass, for the deviation of the rays on account of the refrangibility of the colours, but this deviation may even be removed on account of the spherical forms of the glass surfaces; by which means the effect of achromatic telescopes is much more powerful than that of mirror telescopes. And it is for this reason, and on account of their construction rendering them fit for every species of observations, that at present almost all astronomical observations are performed with achromatic telescopes.

Although the largest achromatic telescopes hitherto used are small, compared with the largest mirror-telescopes, they have in many respects produced more important results than the latter. The best trial of telescopes is, as every one knows, the observation of double stars; in which, the effect of the glass telescopes is decidedly greater than with the mirror telescopes. Thus, for instance, M. Bessel, of Königsberg, discovered with an achromatic telescope made at this place, the object-glass of which is but 48 lines, that the star ζ *Bootis* (stated by Herschel to be a double star of the 4th class) belongs, at the same time, to the 1st class; since it has, besides the principal star, another star near it, which Herschel did not see. In the same manner several fixed stars observed before, have only been found lately to be double stars when observed through achromatic telescopes.

It is a fact well known, that the effect of the telescope lies, not in its length, but in the diameter of the object-glass; so that for instance, among telescopes of a proportionably equal perfection, that telescope whose object-glass is of twice the magnitude of another will also be of double its strength. The difficulties of making large achromatic telescopes proportionably good with small ones, do not increase so much in the proportion of the diameter of the object-glass, as they do in proportion of the cube. This difficulty not having been as yet conquered, the large achromatic telescopes, the object-glasses of which had more than 48 lines aperture, did not bear a proportionate perfection to the smaller ones; and if still

larger,

larger, their power decreased. One of the difficulties was, that the glass used for the object-glass could not be obtained as perfect as such large instruments require. In fact, the English flint-glass has undular lines which disperse the light irregularly in its passage through it. These streaks being more numerous in a larger and thicker glass than in a small one (whilst, if the effect is to be increased, they ought to be less so), the power of the object-glass was diminished if the instruments were particularly large. The English crown-glass too, as in fact every other kind of glass hitherto used, has those undular streaks, which, although not always visible to the naked eye, will yet give a false direction to the rays by an irregular refraction. The Bavarian flint- and crown-glass, however, is free from these streaks, and equally compact throughout: the difference between the flint- and crown-glass being chiefly in the greater power of dispersing the colours, and the proportion of this power being in the English flint-glass, compared to the common glass, as 3 to 2, but in the Bavarian as 4 to 2: the latter is also preferable in this respect in the given proportion.

There were not till the present time any fixed theoretic principles for the construction of achromatic object-glasses: and opticians were obliged, within a certain line, to rely on chance, which made them polish a greater number of glasses, and select those in which the faults were most compensated. As the probability of this chance is much less in large glasses than in small ones, even those of the middle size would have been seldom perfect; and even with the best flint-glass the construction of large achromatic object-glasses would have been impracticable. The more important causes which rendered this process necessary are as follows: The theory of achromatic object-glasses being as yet imperfect; the means formerly applied for ascertaining the powers of refraction and dispersion of colours in the different species of glass, which ought to rest on a firm basis, not being sufficiently established; and on account of the methods hitherto used for grinding and polishing the glasses not being calculated to follow the theory with that degree of exactness, as they ought, if a palpable indistinctness should be avoided.

All those impediments, however, together with many others, have now been successfully removed, partly by inventions and partly by discoveries, to which we were led in pursuing this object. I shall, however, perhaps find another opportunity for entering more largely upon this subject.

The object-glass of the great refractor, of which I am now speaking, has 108 Paris lines aperture, and 160 inches focus.

The power of a telescope may be best seen by a comparison with another directed on the same object. The great impediments in observing with large telescopes are the imperfection of the air, and especially an apparent undulation. These impediments are increased with large instruments, in proportion to the squares of the diameter of the object-glasses, but the effect increases only in proportion to the diameter; whence, although the sky may appear clear, and the air in that respect be but slightly imperfect, no observations can be made with large instruments. As the air is perfect in this respect but few days in the year, we chose for the purpose of ascertaining the proportionate effect of the large telescope, a terrestrial object, fixed for this purpose; as by this means the space of air which we had to look through being smaller, its imperfection would be less injurious. The trials made in this manner have shown that the effect of the great refractor increases, as it should, in proportion to the magnitude of the diameter of the object-glasses. It would lead us too far, were we to enumerate all the means which were employed, for instance, only for bringing the axes of the glasses perfectly into one line, to counteract the contraction and expansion of the metal rims of the object-glasses in different temperatures, &c.; circumstances which we had to attend to, in order to secure the greatest effect of the instrument.

One of the greatest impediments found hitherto in the observation of celestial objects, by means of large telescopes, is the apparent diurnal motion of the stars, which increases in proportion to the size of the instrument; so that the stars lying towards the equator remain but a very short time within the field of view of a strongly magnifying telescope, and traverse it very rapidly. However small the motion that may be given to the instrument by means of screws, and for the purpose of following them, it will receive oscillations which will be larger in proportion to the size of the telescope. Before the instrument has come to rest, the star will have crossed the field of view, so that the observer will see it perhaps only for a few moments, and as it were by accident, under favourable circumstances; circumstances which will be the more rare, as a star is seen to the greatest advantage only in the centre of the field. These difficulties could only be removed if the telescope could be made to follow the stars without the intervention of a human hand, whether their motion be apparently slow, as at the pole, or rapid, as at the equator.

For this reason the telescope has been mounted in a peculiar manner on a parallactic principle; *i. e.* one of the two principal axes, on which it is made to turn, is so inclined towards

wards the horizon, that its inclination may exactly correspond with the latitude of the place, and is consequently directed towards the pole. The second axis, called the axis of declination, is exactly vertical upon the first, or hour-axis. Thus, by directing the instrument upon a star, the hour-axis need only be moved with that degree of velocity, as to make it turn once within 24 hours, like the axis of the earth; by which means the star will always remain in the field as long as it stands above the horizon. This motion is imparted to that axis by means of clock-work, consisting of two distinct parts. The weight of the one part overcomes the friction and inertia of the mass of several hundred weight; the other part regulates the motion. But in order to prevent a concussional motion, and make the same regularly uniform, the clock-work was made without the usual pendulum, or balance. The regulator of this work is a centrifugal pendulum, which, being inclosed in a cone, constantly turns in one direction; and both the parts of the work may be wound up without the motion of the telescope being interrupted in any degree whatever. The telescope may also be stopped, and again set in motion, without any necessity of arresting the movement of the clock-work; and, if required, it may also be moved into any direction, either with the hand or by means of a screw. The motion of the clock may be at any moment accelerated or retarded, by simply moving a spiral disk to a different degree of its division. By this means a star may be moved to the centre of the field of view, which is peculiarly useful in micrometrical observations, and is not practicable in any other manner. By means of this disk we may give the telescope instantaneously the movement corresponding with that of the moon, or any of the planets.

In order to render an uniform motion of the great telescope possible, it must be completely balanced with respect to its two principal axes, in whatever position it may be brought, without however this balancing being an impediment to its being directed towards any point of the sky that may be required. With respect to the axis of declination, the telescope, not being fixed in the centre, is balanced by two weights placed near the eye-glass and fastened to a conical brass tube, each having in the point of gravity two axes intersecting each other at right angles; so that in this respect the telescope is equally balanced in every direction. With respect to the hour-axis, the telescope is balanced by two weights, one of which is fixed immediately on the axis of declination. The second weight is fastened to a bar of a peculiar shape, forming

ing a ring towards the hour-axis. This ring touches(by means of two other axes placed opposite one another) a second and smaller ring; and this ring turns on the case containing the axis of declination; so that also with respect to the hour-axis the telescope is exactly balanced in every direction. In order to prevent the friction of the hour-axis and its pressing on its bed, another weight is added, operating on the bed of two friction-rollers. By all these dispositions the telescope, notwithstanding its immense size, may be moved with one finger.

The pedestal is of such a shape, that, although its position must never be altered, it cannot hinder the telescope from being turned towards any point of the heavens. It seems, indeed, that there may be situations of the telescope, in which the pedestal may be an obstacle against following the star; yet the instrument is so constructed that the telescope may be directed in two ways upon one and the same object, simply by turning the hour-axis 180°. Thus, if the pedestal should be an obstacle on one side, the turning of this axis will render the telescope free on the other.

As it is very difficult with a large telescope to find an object and bring it within the focus, it is usual to add to it a small one, the axis of which is perfectly parallel with that of the large one. The finder of the large refractor has 29 lines aperture, and 30 inches focus.

Each of the two principal axes has a graduated circle, called the hour and declination circles. These are fastened to their axis and turn with them. The division of the hour-circle shows 4 seconds of time, and that of the declination-circle 10 seconds of space. By this means those stars which are out of the meridian may also be found and observed in the daytime, which is particularly useful with fixed stars of the 1st magnitude, which cannot be observed so well in many particulars at night.

There are yet many other parts about this instrument, the use of which can however be illustrated only by a detailed description.

FRAUNHOFER.

Postscript.—The above description was not originally destined for the press, nor was it written for astronomers; it therefore contains much that is superfluous. A detailed description ought to be accompanied with designs of the several parts. I had only drawn the whole of the instrument in a perspective view, from the side on which the clock-work is fixed,

fixed, and a print of which accompanies the present description *.

When the instrument was sent off to Dorpat, the micrometer, &c. belonging to it were not completed; but they will be there before the instrument can be entirely set up.

In the line micrometer belonging to the instrument, both threads may be separately moved by means of a screw; partly for the purpose of placing each thread where it may be required, partly for enabling the observer to make a kind of repetition in the observations with micrometers, which, with the use of the clock-work that moves the refractor, is much more practicable than it is in the common way of mounting. In the same sense the eye-glass is separately moveable, in order to make the two threads stand always equidistant from the centre of the field of view, which makes them both equally distinct. That part of the micrometer containing the threads, supports, besides the necessary correction-screws, &c., two verniers, in opposite positions, moving upon a graduated circle, made for the purpose of measuring the angles of position. The verniers read off to one minute. The micrometer may be gently moved with respect to the position-circle, with the hand or with a screw. The lines only are capable of being illuminated, so as to leave the rest of the field of view quite dark. As the position-circle must remain unalterable with respect to the position-axis, but the micrometer, together with the apparatus for lighting the threads, must be capable of being turned, I was obliged to make a disposition of it different from that which I had hitherto employed with micrometers without a position-circle. The whole field may also be lighted. This micrometer has four distinct eye-glasses.

The refractor will receive moreover a lamp circle-micrometer with four eye-glasses; a lamp net-micrometer with three eye-glasses; and finally, four ring-micrometers, two of which contain double rings.

As distinctness can only be properly obtained by the axis of the object-glass and that of the eye-glass being exactly in the same line, and a deviation in this respect being more injurious in large object-glasses than in small ones, a particular instrument will be added to the refractor, by which this deviation may be found and corrected.

FRAUNHOFER.

* This plate is inserted in the *Astron. Nach.*, and it may also be seen in the 2d volume of the Memoirs of the Astron. Society of London; but it is too large for insertion in this work.

VI. *On*

VI. *On the Manner of estimating the Difference of Longitude in Time.* *By* A CORRESPONDENT.

NO part of astronomy is more abstract, or more liable to error, than what regards computations in time. In the *Conn. des Tems* 1825, M. Bouvard has calculated the difference of longitude between Paris and Greenwich, by means of the moon's observed motion in right-ascension in the time elapsed in passing between the two meridians. This calculation is animadverted on in the Quarterly Journal of Science published in March last; and as I observe that, in the discussion, what is true is mixed with notions not quite correct, it may be permitted to bestow a few lines on the subject.

M. Bouvard's method of calculation may be thus explained. Conceive a fictitious sun moving equably in the equator at the rate of 360° 59′ $8''\frac{1}{3}$ in 24 hours, and consequently marking mean solar time by the arcs which it describes. Let the fictitious sun and the moon be upon a meridian at the same instant of time; and, after a given interval elapsed, the moon having come to a second meridian, the fictitious sun will be past that meridian. It is manifest that the time of describing the arc of the equator between the fictitious sun and the second meridian is equal to the moon's variation in right-ascension estimated in mean solar time. If the moon's change of right-ascension in sidereal time be equal to a, and the ratio of sidereal to mean time be equal to r, then the mean time of describing the arc of the equator between the fictitious sun and the second meridian will be equal to ar. Again, let $m = \frac{59'\ 8''\frac{1}{3}}{24}$; then $15° + m$ will be the rate at which the fictitious sun separates from the first meridian in an hour of mean time: and if h be the moon's motion in right-ascension in an hour of mean time, then $ar \times \frac{15° + m}{h}$ will be the arc of the equator between the fictitious sun and the first meridian expressed in mean time. Hence the mean time of describing the arc between the two meridians is equal to

$$ar \times \frac{15° + m}{h} - ar = ar\left(\frac{15° + m - h}{h}\right).$$

This is M. Bouvard's formula; but it certainly is not the difference of longitude sought. For, *according to the definition of the term*, the difference of longitude is found in time by converting the arc between the two meridians at the rate of 15° to 1^h; and this is true whether we use mean time, or sidereal time, or any other time, provided the interval between one

one passage of the meridian and the next following be divided into 24 hours. Therefore one step more is necessary for finding the true difference of longitude; namely, to change the arc from hours of mean time, to hours at the rate of 24 to 360°, which is done by dividing by r. Hence, if d be the difference of longitude, we have,

$$d = a\left(\frac{15^\circ + m - h}{h}\right).$$

This is Mr. Henderson's formula in the Quarterly Journal.

If we suppose that a star is upon the first meridian at the same instant with the moon, the star will be past the second meridian when the moon arrives upon it; and the arc of the equator between the star and the second meridian is evidently the moon's variation in right-ascension, and equal to a in sidereal time. The star separates from the first meridian 15° every hour of sidereal time, and $\frac{15^\circ}{r}$ every hour of mean time; wherefore the whole arc of the equator between the star and the first meridian, is equal to $a \times \frac{15^\circ}{h \,.\, r}$ in sidereal time. Hence the sidereal time of describing the arc between the two meridians is equal to

$$a \times \frac{15^\circ}{h \,.\, r} - a;$$

and, as this time is at the rate of 1^h to 15°, we have

$$d = a \times \frac{15^\circ}{h \,.\, r} - a.$$

This is Mr. Henderson's second formula, and it is precisely the same with the first, since $\frac{15^\circ}{r} = 15^\circ + m$.

The longitudes ascertain the relative positions of the terrestrial meridians. If this end is to be accomplished by estimations in time, it is requisite that the intervals elapsed be proportional to the arcs of the equator. The difference of the times of passing any two meridians must be the same part of an entire revolution that the intercepted arc of the equator is of the whole circumference or 360°. If the time of a whole revolution of the heavens be divided into 24 hours, there will be the same number of hours in the difference of the longitudes of two given meridians, whether the hours be long or short, whether they be mean solar hours or sidereal hours. The question relates entirely to different ways of measuring the same quantities; exact proportionality in the measures is alone required; their absolute magnitude is not considered.

I cannot therefore subscribe to the decision *ex cathedrâ*, which appears in p. 121 of the Journal, namely,

"It must be observed that when a difference of longitude is expressed in time, the time intended is sidereal, and not solar."

If any difficulty should still remain after what has been said, here is an authority that ought to have some weight.

"La difference des méridiens en tems est toujours la difference du tems que l'on compte à un instant donné, soit que les observateurs emploient le tems vrai, le tems moyen ou le tems sidéral, pourvu que les deux observateurs emploient le même tems."—*Delambre's Astron.* vol. ii. p. 203.

In the *Requisite Tables* there is a list of places with their longitudes, reckoned from Greenwich, expressed both in degrees and in time. But I find no intimation that the time meant is sidereal and not solar time. Yet, from the known precision of the estimable author of the Tables, who presided so long and with so much credit over the astronomical science of this country, there is no doubt that such an intimation would have been given, if it could have contributed in any degree to remove misapprehension, or to guard against error. If I have a clock regulated by sidereal time, and wish to know the sidereal time at the meridian of any place in the Tables, I apply the difference of longitude in time to the time of my clock, and I have what I desire. If my clock is regulated by mean time, I follow exactly the same procedure, with the like success. In the first instance, the difference of longitude is in sidereal time; and in the second, in mean solar time. It is always in that time according to which one reckons.

July 18, 1825. DIS-IOTA.

VII. *Defence of Mr.* J. HERAPATH'S *Demonstration of the Binomial Theorem. By* A CORRESPONDENT.

To the Editor of the Philosophical Magazine and Journal.

Sir,

MR. WARD has objected in your last to Mr. Herapath's demonstration of the binomial theorem, published in the preceding Number.—It had been shown by Mr. H. that

$$n,\ \frac{n-1}{2},\ \frac{n-2}{3},\ \frac{n-3}{4},\ \ldots\ \frac{n-(m-1)}{m} \qquad \text{(B)}$$

are respectively the quotients of the second by the first, the third by the second, the fourth by the third, &c. to the $(m+1)$th by the mth coefficients of the expansion of $(x+y)^n$; and hence he immediately concluded that the qth coefficient is the product of $q-1$ of these terms, the coefficient of the first term being

being unity. For not detailing in minutiæ this very obvious and easy consequence, Mr. Ward pronounces Mr. Herapath's demonstration to be "no proof." Let us see the amount of this charge. Employing Mr. H.'s notation, namely,

$$1,\ 2_n,\ 3_n,\ 4_n,\ \ldots\ q_n$$

for the binomial coefficients, the quotients (B) are equivalent to

$$\frac{2_n}{1},\ \frac{3_n}{2_n},\ \frac{4_n}{3_n},\ \ldots\ \frac{q_n}{(q-1)_n}$$

Now it is evident that there are here $q-1$ terms, and that the product of them all is q_n, which is the qth coefficient, as Mr. H. says, p. 323. This is Mr. Ward's difficulty; for not minuting which he finds fault with a demonstration much surpassing in simplicity, brevity, and completeness any other that has been given of the binomial theorem.

Had Mr. H. prefixed 1 to the quotients (B), and said that the qth coefficient is the product of q of them, it might have been considered by some an improvement.

It is evident that in Mr. H.'s proof q may be 2, 3, 4, ... or any whole number greater than 1, which shows that Mr. Ward has likewise been too hasty in his observation, that it must be "any whole number greater than 2."

A Correspondent.

VIII. *Notices respecting New Books.*

Zoological Researches in Java and the neighbouring Islands. By Thomas Horsfield, M.D. F.L. & G.S. Nos. VI. to VIII. London, 1823, 1824. 4to.

FROM the aspect which the study and pursuit of zoology now wears in this country, it would seem as if our naturalists, sensible of the neglect which this important and beautiful department of the investigation of nature has hitherto experienced from us, were determined, by rapid and extensive advances, to overtake and compete with on equal terms, if not in their turn to excel, our zealous continental neighbours. Those indefatigable inquirers have too long given us the law in many branches of this science; but the results of their researches, splendid as they are, are now becoming the foundations for a superstructure of unequalled grandeur and exquisite proportions, raising by our own countrymen, which will present the science of animated nature in a form far more attractive, and far more useful to mankind, than could have been hoped for, a few years back, by its most sanguine votaries.—We allude of course, first, to the extensive collection of facts

respecting the organization of the various subjects of the animal kingdom, and its adaptation to their functions and respective stations in nature, with the distinctions of their more comprehensive groups, which have been furnished chiefly by the industry of the naturalists of Sweden, of France, and of Germany; and secondly, to the discoveries in and refined views of natural arrangement,—of the twofold order of affinity and analogy, now demonstrated to pervade nature,—for which those facts and characters have served as a basis, in the hands of our own naturalists of the present day. The doctrines of the quinary distribution of nature and the circular succession of affinities, shown by Mr. W. S. MacLeay to prevail throughout the insect world, have received, we think, in their successful development in the feathered creation by Mr. Vigors, an unimpeachable confirmation. The new form,—we might almost say the new life,—which these doctrines have imparted to zoology, are comparable only with the benefits which have accrued to the sciences of mineralogy and chemistry, from the knowledge of the mathematical laws of crystallization, and the doctrine of definite proportions.

Nor are the exertions which are making in this country to encourage the pursuit of zoological science, and furnish means for studying it in its various relations, disproportioned to the advances the science is making. Two years have not elapsed since a few zealous cultivators of zoology established, under the name of the Zoological Club, a class of the Linnæan Society expressly devoted to its promotion; and the extent and variety of their labours may readily be appreciated, from the space they occupy in the lately-published part of the Society's Transactions.—Shortly afterwards was commenced a Journal exclusively appropriated to the same branch of knowledge; and this has continued, with increasing interest, to advocate its cause and augment its resources. An ample share of attention is allotted to zoology in the scientific journals of our northern metropolis; and if we may judge from the specimen which the first number of the new Dublin Journal presents, our scientific brethren in that capital are equally alive to the support now required by zoology, and the kindred sciences, from all the cultivators of natural knowledge.—But the most important step of all, perhaps, is the proposed establishment of an Institution, designed especially for the advancement of zoology in one of its most important relations,—the improved application of the different races of animals to the uses of civilized society. Particulars of this noble project, so worthy of its distinguished authors, will be given in a future page of this number; and it is quite unnecessary for us to dilate upon the extensive

extensive benefits which must accrue to the science when it shall be carried into effect.

Such then being the present state and such the future prospects of this science, it is with much satisfaction that we have again to review the labours of a naturalist, who is an active contributor in all the undertakings to which we have alluded; and who, combining with some of the new views of zoological philosophy his own acumen and minute accuracy, and comprehensive acquaintance with the habits and natural relations of the subjects of his researches, derived from contemplating them in their native clime, has produced a work of commensurate excellence in all respects.

The first five numbers of Dr. Horsfield's zoological Researches have already been noticed in the Philosophical Magazine *; but in reviewing the contents of those now before us, which complete the work, we must recur to two or three subjects described in them, for the purposes of adverting to the first application of the distinction between affinity and analogy to the discrimination and distribution of the *Mammalia*, and noticing the author's recent establishment of a new and interesting subgenus of Ursus, of which the *U. malayanus*, described in the fourth number of these Researches, forms a species.

The graphical illustrations of the present numbers, consisting of twenty-four coloured plates of as many animals, and three engravings of anatomical details, are of similar excellence to those in the previous numbers, and have been executed by the same artists,—Messrs. W. Daniell, W. Taylor, and A. Pelletier.

In the enumeration of the mammiferous animals described, we shall adopt the order proposed by Dr. Horsfield for their arrangement in the volume, which is nearly that of Cuvier. The first we have to notice is *Semnopithecus Pyrrhus*, thus characterized:—S. rufus nitore splendide-fulvo, pectore abdomine artubus intrinsecus caudæque basi subtus pallide-flavis. —*Lutung* of the Javanese.

This species agrees with the *S. maurus*, described in a preceding number of the work, in all points except the external covering; the permanency of the difference in which, and the peculiar name of the animal among the natives of Java, appear chiefly to have determined the author to regard it as a distinct species. In the *S. maurus* the fur is intensely black; in the present animal it is reddish-brown, with a beautiful golden gloss on the back, head, tail, and extremities.

The *Vespertilionidæ* figured and minutely described amount to four in number, besides many others of which specific cha-

* See vol. lxii. p. 221.

racters

racters and brief accounts are given: they belong to the genera *Cheiromeles*, *Rhinolophus*, and *Vespertilio*, the first of which is newly established by the author.

Cheiromeles. CHAR. ESSENT. *Dentes primores* utrinque duo; supra magni, approximati, semiconici, acuti, infra minimi, simplices. *Rostrum* conicum, sulcatum, glandulis confertis setiferis in paribus tribus oppositis coronatum. *Auriculæ* distantes, patentes, operculo brevi, semicordato, obtuso. *Saccus* axillaris amplus, ad regionem hypochondriorum extensus, antice membranâ pectorali, postice patagio complexus. Scelidum *podaria* manus sunt. Halluce ungue lamnari, extrorsum serie secundâ setarum uncinatarum marginatâ.

C. torquatus. C. collo pilis longiusculis cincto, dorso punctato nudo.

This curious bat is one of the subjects added to the museum of the East-India Company by the researches of the late Dr. Finlayson, who collected various species of the family in Penang and Singapore. In its physiognomy it bears considerable resemblance to *Molossus*, and on first view was considered as a species of that genus; but a more careful examination evinced that it was a new genus, and more nearly allied to *Nyctinomus*. The singular structure of the foot, or rather according to Illiger's language, of the *podarium*, which in the posterior members has the characters and properties of a hand, the thumb being a complete antagonist to the fingers, suggested the name of *Cheiromeles*,—from χειρ (*manus*) and μελος (*membrum*). About the neck is an irregularly defined collar, or ruff, of long soft hairs, from which the specific name has been taken. The extent of the wings is nearly two feet.

Rhinolophus larvatus. R. supra ex fusco fulvescens postice saturatior subtus fulvus nitore canescente, caudâ pedibus breviore, auriculis magnis acutis erectis approximatis basi latissimis simplicibus.—*Lowo-sumbo* of the Javanese.

Rhinolophus nobilis. R. supra canescente fuscus subtus dilutior, lateribus colli et abdominis axillis maculâque infra-scapulari albentibus, caudâ pedibus longitudine æquali, prosthemate supra membranâ transversâ porrectâ infra acumine elongato, auriculis erectis acutis magnis basi latissimis lobo utrinque subinvoluto.—*Kêbblêk* of the Javanese.

The individuals composing the genus *Rhinolophus*, one of the most remarkable in the family of *Vespertilionidæ*, are peculiarly distinguished by the presence of two papillary tubercles, situated on the os pubis, and exhibiting the external appearance of breasts, which were first distinctly noticed by Bechstein; but which, according to M. Kuhl's observations, although connected with the propagation of the species, are not breasts, as he never discovered lactiferous glands near them.

them. Dr. Horsfield divides the Javanese species into two sections; the first having the superior lobe of the nasal membrane erect and lanceolate—the second having above a transverse membrane, stretching forward as a small arch.

The species included in the first section are *R. affinis* and *minor:* those of the second are *nobilis* and *larvatus* above characterized, *vulgaris*, *deformis*, and *insignis*.

Vespertilio Temminckii. V. capite cuneato supra lateribusque planis, auriculis capite brevioribus oblongis rotundatis margine exteriore parum excisis trago elongato falcato, vellere sericato pilis brevissimis supra fuscis subtus sordide-flavis lateribus capitis corporisque nitore dilute rufescente.

Of this species, peculiarly characterized by the shortness of its fur, a detailed account is given; with concise notices of the following; all Javanese:—*V. adversus*, *Hardwickii*, *tralatitius*, *imbricatus*, and *pictus:* all of which, except the last, are new.

Passing now to a very different family of *Mammifera*, we have to notice Dr. Horsfield's recent establishment of a subgenus of *Ursus*, from the *U. malayanus*, described in the fourth number of this work, and a new species from Borneo, at present exhibiting in the menagerie at the Tower*. His memoir on the subject is contained in the sixth number of the Zoological Journal; but the first species discovered of the new group having been described in the Researches, a notice in this place will be most appropriate.

To the new subgenus Dr. H. applies the name of *Helarctos*, from ἕλη (*calor solaris*) and αρκτος (*ursus*), its range appearing to be limited within a few degrees of the equator. The following extracts from his detailed subgeneric character will be sufficient for our present purpose.

Char. Subgen.—*Dentes primores* et *laniarii* iidem qui aliis hujus generis speciebus.—*Molares* supra utrinsecus quinque: tres anteriores unicuspides; primus majusculus laniariis approximatus, secundus minimus occultatus, tertius mediocris; quartus et quintus tritorii, coronidibus oblongis compressis tuberculatis, magnitudine reliquis hujus generis speciebus vix æquantibus. Infra utrinsecus quinque; anteriores tres unicuspides, duo posteriores tuberculati, oblongi, magni.—*Lingua* gracilis, longissima, extensilis basi papillis planis compressis obsita, apice lævâ.

The species are, *malayanus* (*Ursus malayanus*), *H.* ater,

* Whilst this article was passing through the press, a living specimen of *Ursus malayanus* itself has been received from Sumatra by Sir Stamford Raffles, and consigned provisionally to the above menagerie, where it may be compared with its congener, and the validity of Dr. Horsfield's specific distinction of the two satisfactorily ascertained.

pectore

pectore maculâ semilunari albâ;—and *euryspilus*, *H.* ater, pectore plagâ amplâ aurantiâ supernè profundè emarginatâ, pedibus fasciâ transversâ cinereâ. The former is figured in the Researches, and the latter in the Zoological Journal, where it is described at length.

Proceeding from the *Plantigrada* to the *Digitigrada* we first come to

Viverra Rasse.—V. griseo-fulvescens, auriculis subapproximatis, dorso lineis octo longitudinalibus parallelis nigricantibus vario, collo fasciis obscuris, pedibus concoloribus fuscis, pilis corporis caudæque attenuatæ rigidiusculis.—*Rasse* of the Javanese.

At once distinguished from the other species of this genus, as defined by Cuvier, by its lengthened form, and by the slenderness of all its parts, the *Viverra Rasse* supplies in Java the place which the *V. Civetta* holds in Africa, and the *V. Zibetha* or *Tanggalung* of the Malays on the Asiatic continent, from Arabia to Malabar, and in the large islands of the Indian Archipelago. From the latter it differs as much in its natural disposition as in external characters. The *Tanggalung* is an animal comparatively of a mild disposition; it is often found among the Arabs and Malays who inhabit the maritime parts of Borneo, Macassar, and other islands, in a state of partial domestication, and, by the account of the natives, becomes reconciled to its confinement; and in habits, and degree of tameness, resembles the common Domestic Cat. The *Rasse*, on the contrary, preserves in confinement the natural ferocity of its disposition undiminished; and never propagates in this state. The odoriferous substance obtained from it, which is a favourite perfume among the Javanese, agrees with the civet afforded by the other species, in colour, consistence, and odour.

Mangusta Javanica. M. fusco-nigricans, nitore glaucino undulatâ tæniolisque fulvo-cinereis, variegatâ, capite dorso pedibusque saturatioribus, caudâ attenuatâ apice simplici acutâ.—*Garangan* of the Javanese.

In his details respecting this animal, Dr. Horsfield applies the principles of natural arrangement first developed by Mr. W. S. MacLeay, to the discrimination of the Feline and Viverrine animals; and as we believe this to be the first application of those principles to the highest class of the *Vertebrata*, we shall extract his statement on this subject.

"The examinations connected with the description of the *Mangusta javanica*, and the comparisons which I instituted among the numerous genera above mentioned, naturally suggested a reconsideration of the *Felis gracilis*, which was described

scribed in the first number of these Researches: and as the most rigorous comparisons and examinations have confirmed my original views, regarding the natural affinity of this animal, I shall now concisely detail their result.

"For the purpose of following me in one of the most essential points of this description, I beg the reader to take before him the plate of illustrations contained in the first number, and that which is annexed to the present number, of these Researches. It will then be perceived that between the grinders of the *Felis gracilis*, and of the *Mangusta javanica*, there is a great resemblance. In the grinders of the upper jaw of the *Felis gracilis* this resemblance appears, indeed, only partially. The view, which, in making the delineation, was taken from the specimen prepared for the Museum, did not exhibit the tuberculous tooth with the minuteness of detail that would have been desirable; and what appears a single posterior tuberculous tooth will probably, on an inspection of the naked cranium, be found to consist of two teeth placed in close contact: but the contour and separate parts of the other teeth are given with a degree of accuracy sufficient for my present purpose. In the grinders of the lower jaw this resemblance is strong and apparent: thus, with the exception of the additional heel of the base of the first three false grinders, these teeth, in both animals, are perfectly similar; and in the most characteristic tooth, the fifth grinder, being the true *carnassier*, or carnivorous tooth, it is difficult to perceive any difference whatever. But if the comparison be extended to the front teeth of these two animals, they will be found to be perfectly dissimilar. The front teeth of *Mangusta javanica* have all the characters of the Viverrine animals enumerated; namely, *Genetta*, *Viverra* (as above limited), *Suricata* (agreeably to Desmarest and Illiger), and *Mangusta*; while those of the *Felis gracilis* will be found strictly to agree with the teeth of Feline animals. After these remarks relating to the teeth alone, I proceed to compare the other characters of *Felis gracilis* with those of Viverrine animals generally. The *Felis gracilis*, exclusive of the lengthened muzzle and slender body, has the general physiognomy of Feline animals. Its claws are completely sheathed and retractile, and its feet have precisely the same covering, disposition, and attitude as those of Cats; with this difference, that *Felis gracilis* has five toes to the hind feet, while the genus *Felis*, as previously known, has only four. To afford a distinct view of this most essential character, care has been taken both in the plate of details and in the figure of *Felis gracilis*. This animal further agrees with the Feline tribe,

in the absence of the anal folliculus, or pouch. Of this I am enabled to judge, by the complete absence of all odour during the period I had it in confinement. The Viverrine animals hitherto known, and particularly the *Mangusta javanica*, have long, naked, horny claws, which, although defined semi-retractile, and semi-vaginate, possess this property in a very slight degree, and are more properly fitted for burrowing in the earth, which many of them, and particularly the *Mangustæ*, perform with great dexterity. They have, without exception, either simple follicules, gradually larger in size in the successive genera, according to the order in which they have been enumerated, or an extensive anal pouch or sack. Their tail is in a greater or less degree acuminated, which contributes, with their other characters, to give them a physiognomy essentially different from that of Feline animals.

"From these details it appears, that although the *Felis gracilis* agrees with the *Mangusta javanica* in the structure of the grinders, particularly of those in the lower jaw, it is in other respects entirely different, as well from this animal, as from Viverrine animals generally; while, with a slight modification of characters, it agrees with the Feline animals hitherto known, particularly in the general physiognomy, in the structure and disposition of the feet and claws, in the absence of an anal pouch, in a cylindrical tail, and even in the external marks on the skin. We have thus an agreement in one character, or a relation of analogy, while in the aggregate of the other characters, which should regulate us in the classification of the animal, we have an indication of its relations of affinity.

"When I examined the *Felis gracilis*, preparatory to the description which has been given in the first number of the Zoological Researches, these different relations occasioned me some perplexity. After carefully balancing its characters, and exhibiting a tabular view of the genera *Felis* and *Viverra*, as defined by Illiger, to show at one view its relations to both these genera, I finally determined, by a preponderance of what appeared to me its natural character, to associate it with the genus *Felis*. This indeed appeared to me more a family than a genus; but the peculiarities of the *Felis gracilis* made it necessary to construct and define a distinct section, for which I proposed the name of *Prionodontidæ*. When I discovered the *Felis gracilis* in the forests of Blambangan, before I had considered the numerous subdivisions which the genus *Viverra*, as employed by Gmelin, requires, I included it in that genus in a catalogue of *Mammalia*, which I forwarded to England in the year 1812; but a more careful examination has convinced

vinced me that it cannot be separated from Feline animals, however peculiar in some characters, without violating its natural affinities.

"Having already stated that the discussion of the characters of the *Mangusta javanica* almost insensibly led me to a reconsideration of those of the *Felis gracilis*, I can now add, that several of the difficulties which presented themselves, when the first number of these Researches was prepared, in the classification of this animal, from the existence of those characters which rendered it doubtful whether it were a *Felis* or a *Viverra*, have in a great measure been removed by the views to which I have been directed by the *Horæ Entomologicæ* of my learned and highly respected friend W. S. MacLeay, Esq.; in which, from an admirable extent of observation and research, and with consummate originality, he has explained and illustrated the various relations of animals. I shall, therefore, agreeably to the direction afforded to me by this work, consider the *Feline* animals as one series, and the *Viverrine* animals as another series. That the animals of the former series are still imperfectly known can clearly be demonstrated. As one proof, I shall adduce the *Felis capensis* of Forster. This distinguished naturalist, who was not unacquainted with the characters of *Felis* and *Viverra*, as defined by Linnæus, clearly gives, as a property of the animal described by him, a *lengthened head.*—Phil. Trans. vol. lxxi. p. 5. *Caput rostro magis acuto quam Felis cati.* And M. Desmarest, in describing this animal in the *Encyclopédie*, offers in a note the following remark:—"M. Georges Cuvier, au quel on doit les rapprochemens que nous adoptons, a lui-même balancé à regarder le chat du Cap, *Felis capensis* de Forster, comme une vraie civette. Dans son *Mémoire sur les Espèces de Chats*, il le considère comme une espèce voisine du chat serval; mais dans son dernier ouvrage (*Le Règne Animal*), il dit qu'il ne diffère pas de la genette." Concerning the *Viverra tigrina* of Schreber, which was figured by Vosmær, it more resembles a Feline than a Viverrine animal. M. Desmarest points out its affinity to the *Felis gracilis;* and the name given it by Vosmær, *Chatbizaam*, shows that he considered the characters of the Cat to predominate. The specific character also exhibits a remarkable agreement with our animal in the external marks, at the same time that it clearly indicates a specific distinction. We have thus in the *Felis capensis*, and in the *Viverra tigrina* Gmel., two animals of the Feline series, which are analogous to Viverrine animals; and it may reasonably be expected that future discoveries will make known others, which will unite the *Felis gracilis* with the Feline animals now known, and thus

complete the series which is still interrupted. In the Viverrine animals, which are apparently better known, the series is more regular. According to the relative development of the grinders, it has been arranged above in the following order: *Genetta*, *Viverra*, *Suricata*, *Mangusta*. The proportional development of the anal folliculi also confirms this disposition: in *Genetta*, it constitutes a simple excavation; in *Viverra* it is a pouch, divided into two sacks; in *Suricata*, and particularly in *Mangusta*, it consists of an extensive portion of integument, with numerous folds, which passes over and incloses the anal aperture.

" These observations, which show the regularity of the Viverrine series, and the interruption still existing in the Feline series, were necessary to my conclusion, that although the *Felis gracilis* agrees with the *Mangusta javanica* in its relations of analogy, the aggregate of its characters, which constitute its relations of affinity, associate it with *Felis*. In illustration of these observations, I shall introduce Mr. MacLeay's words: 'The test of a relation of affinity is its forming part of a transition continued from one structure to another, by nearly equal intervals; and the test of a relation of analogy is barely an evident similarity in some *one or two remarkable points* of formation, which at first sight give a character to the animal, and distinguish it from its affinities. As a relation of analogy must always depend on some marked property, or point of structure, and as that of affinity, which connects two groups, becomes weaker and less visible as these are more general, it is not at all surprising that what is only an analogical correspondence in one or two particulars, should often have been mistaken for a general affinity.'—*Horæ Entom.* vol. i. pt. 2. p. 364.—These remarks explain at once, and remove the difficulties which have occurred in the classification of the *Felis gracilis*. By regarding its relations of analogy alone, which I presume I have observed in the grinders, and particularly in those of the lower jaw, it has (as far as appears to me) improperly been classed with Viverrine animals; and it has therefore been my endeavour to show that its relations of affinity associate it with Feline animals.

" Directed and confirmed by these views, I shall in future consider the *Felis gracilis* as the type of a distinct genus in the *family* of Feline animals, and designate it by the name of *Prionodon*, which has already been proposed for a section of the *genus* Felis. This name accords with the character of the teeth. Comparatively with those of other Feline animals, the teeth are not only more numerous, but they are more compressed and elongated; their crowns are more strongly notched

or

or serrated, exhibiting numerous acute points; and a lateral view of them may aptly be compared to a saw, the teeth of which are jagged or uneven. The name is accordingly compounded of two Greek words (πριων and οδους), explaining the character of the teeth."

The remaining Quadrupeds described in this work are as follows:

Lutra leptonyx. L. fusca, nitore fulvo, gula sordide flavescente, caudâ corpore dimidio breviore, unguibus brevibus obtusis sublamnaribus.—*Welingsang* or *Wargul*, of the Javanese.

Mus setifer. M. caudâ annulosâ elongatâ, corpore setoso nigricante-fusco subtus cano, dorso setis suberectis rigidis hirto, uropygio setis longis æqualibus postice spectantibus obtecto, auriculis magnis rotundatis nudiusculis.—*Tikus-wirok*, of the Javanese.

Sciurus Plantani. S. supra fulvo fuscoque varius, subtus circulo oculos cingente strigâque utrinque laterali fulvis, caudâ corpore paululum longiore nigro annulatâ.—*Bajing*, of the Javanese: Plantane Squirrel of Pennant and Shaw.

Sciurus bicolor. S. supra niger, infra fulvus, auriculis acutis imberbibus, palmarum ungue pollicari magno rotundato. *Jelarang*, of the Javanese.—*S. bicolor*, Sparrman, Gmelin.—Var. β in insulâ Javâ frequentissima.—S. supra fuscus, varians a fusco-nigricante ad sordide-fulvum, pilis velleris fulvis et canescentibus intermixtis, subtus fulvus vel pallide flavescens.

The description at large of *S. Plantani* is followed by a general enumeration of Indian *Sciuri*, comprising the following species:—*nigrovittatus* (new), *albovittatus*, *bivittatus*, *insignis*, *Palmarum*, *Finlaysonii*, *affinis*, *tenuis* (new), *erythræus*, *Leschenaultii*, *Prevostii*, *hypoleucos* (new), *macrourus*, and *maximus*.

We must now make an abrupt transition to the *Pachydermata*, of which we find *Rhinoceros Sondaicus* described. This species was originally separated from the *R. unicornis* v. *indicus* by Cuvier. Dr. Horsfield contributes to its illustration some remarks, made in 1817, on a living specimen kept in a state of partial domestication at Surakarta, the capital of the dominions of the Emperor of Java, with a good figure of it, from a drawing by an able artist educated in the country. He characterizes the animal as follows:—Rhinoceros cornu unico, rugis colli obsoletis, scutulis epidermidis margine angulatis medio concavis setis paucis brevibus obsitis, auribus margine caudâque subtus pilosis.—*Warak* of the Javanese: *Badak* of the Malays, and of the inhabitants of the western parts of Java.—We extract some interesting particulars of the habits of this animal.

"The individual which is represented in our plate, and which

which has afforded the preceding details, was taken, while very young, in the forests of the province of Keddu, and was conveyed to the residency at Magellan, in the year 1815 or 1816. By kind treatment it soon became domesticated to such a degree, that it permitted itself to be carried in a large vehicle resembling a cart to the capital of Surakarta. I saw it during its conveyance, and found it perfectly mild and tractable. At Surakarta it was confined in the large area or square which bounds the entrance to the royal residence. A deep ditch about three feet wide limited its range; and for several years it never attempted to pass it. It was perfectly reconciled to its confinement, and never exhibited any symptoms of uneasiness or rage, although, on its first arrival, harassed in various ways, by a large proportion of the inhabitants of a populous capital, whose curiosity induced them to inspect the stranger of the forest. Branches of trees, shrubs, and various twining plants were abundantly provided for its food; of these the species of *Cissus*, and the small twigs of a native fig-tree were preferred. But plantains were the most favourite food; and the abundant manner in which it was supplied with these, by the numerous visitors, tended greatly to make the animal mild and sociable. It allowed itself to be handled and examined freely, and the more daring of the visitors sometimes mounted on its back. It required copious supplies of water; and when not taking food, or intentionally roused by the natives, it generally placed itself in the large excavations, which its movements soon caused in the soft earth that covered the allotted space. The animal rapidly increased in size: in the year 1817, having been confined at Surakarta about nine or ten months, the dimensions, as already stated, were nine feet in length, and four feet three inches in height at the rump. In 1821 it had acquired the height of five feet seven inches. This information I received from my friend Mr. Stavers, who is now in England, on a visit from the interior of Java; and he favoured me further with the following details, which complete the history of the individual whose figure is annexed to this article. Having considerably increased in size, the ditch of three feet in breadth was insufficient for confining it; but leaving the inclosure, it frequently passed to the dwellings of the natives, destroying the plantations of fruit-trees and culinary vegetables, which always surround them. It likewise terrified those natives that accidentally met with it, and who were unacquainted with its appearance and habits. But it showed no ill-natured disposition, and readily allowed itself to be driven back to the inclosure, like a Buffalo. The excessive excavations which it made by continually wallowing in the mire, and the accumulation

lation of putrefying vegetable matter, in process of time became offensive at the entrance of the palace, and its removal was ordered by the emperor, to a small village near the confines of the capital, where, in the year 1821, it was accidentally drowned in a rivulet.''

Of the *Ruminantia* we also find but one species described:—

Cervus Muntjak.—C. cornibus caule elongato insidentibus basi bipartitis ramo altero elongato subcontorto apice uncinato altero brevi acuto, laniariis exsertis longissimis, sinubus lacrymalibus maximis, facie rugosa sulcato.—*Muntjak* in the Sunda language, *Kīdang* in Javanese, and *Kîjang* in Malayan: the *Chevreuil des Indes* of Allamand and Cuvier, *Rib-faced Deer* of Pennant and Hamilton, and *Cervus Muntjak* of Zimmerman.

This animal, perhaps the most elegant and graceful of the genus, holds in the Indian islands the same place which the *Cervus Capreolus*, or Roe, occupies in Europe; it also resembles that species in form and general proportions, but exceeds it in dimensions, and surpasses it in agility and sprightliness. With the Malayan poets it is the emblem of swiftness and wildness. The peculiar character of the horns distinguishes it at once from all other species of *Cervus*. The horns of the adult *Kīdang*, in a perfect state, consist of one principal branch, with a smaller additional antler rising on the same base, from the coronal margin of the pedestal, and projecting forward and inward: the tubercles which occur on the horns in most species of the Deer are wanting, except in the horns of the first growth, which are nearly covered with small tubercles.

Dr. Horsfield shows that the *C. moschatus*, and *C. subcornutus* of De Blainville were established from crania of the *Muntjak*, the latter from an imperfect one; and accordingly gives those names in his list of synonyms. Notwithstanding the resemblance in many points which this animal bears to the Roe, it differs from it in several essential particulars; whilst by its canine teeth and the great size of its lacrymal furrows, it is connected with *C. Elaphus*.

We must postpone the characters of the Birds described in this work until our next Number. [To be continued.]

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Zoological Journal. No. VI.

This number contains the following papers:—On two new genera of Birds (*Laniadæ*), *Formicivora* and *Drymophila*, by Mr. Swainson.—Description of a new genus of Mammiferous Quadrupeds of the order *Edentata*, by Dr. Harlan, of Philadelphia, with a figure. The animal described in this paper, named *Chlamyphorus truncatus*, is a very curious addition to South-

South-American Zoology, and is thus characterized by Dr. Harlan:—"Corpore, supra testâ coriaceâ, postice truncatâ, squamis rhomboideis, lineis transversis dispositis, conflatâ, subtus capillis albis, sericeis, obtecto; capite supra squamis testâ dorsali continuis, adoperto; palmis plantisque pentadactylis; unguibus anterioribus longissimis, compressis; marginibus externis, mucronibusque acutis; caudâ rigidâ, sub abdomine inflexâ."—Inquiry into the true nature of Instinct, by Mr. French, Essay iii. continued; comprising these subjects,—Analysis of Dr. Hancock's theory of Brute Action; Influence of the Human Mind upon Brutes; Anecdotes of Human Influence upon Brutes; Essential and Distinctive Attributes of Man.—Sketches in Ornithology, by Mr. Vigors, continued, the present subject being the genus *Icterus* of Brisson.—Mr. Broderip On two new shells from the Mauritius.—Mr. Bell On a new genus (*Amblyrynchus*) of *Iguanidæ*.—Tabular view of the genera composing the class *Cirripedes*, &c. by Dr. Leach.—Account of the Mode in which the Boa Constrictor takes its prey, by Mr. Broderip.—Dr. Horsfield's Description of the *Helarctos euryspilus**.—Descriptions of new or rare subjects in Zoology, by Mr. Vigors, with figures by Mr. J. Sowerby: including *Anthropoïdes Stanleyanus*, from India, and eleven new species of Coleopterous insects.—Mr. Gray On the division of the *Vespertilionidæ* into groups.—Mr. G. B. Sowerby On a new genus (*Octomeris*) of *Cirripedes*. — Analyses of Books, among others, of Spix's *Species Novæ Testudinum, Ranarum, et Serpentum Brasiliensium*.—Zoological Proceedings of Societies.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

June 17.—AN extract of a letter was read from John Kingdom, Esq.; communicated by Joseph Townsend, Esq. F.G.S.

Mr. Kingdom mentions in this letter the situation in which certain bones of a very large size, appearing to have belonged to a whale and a crocodile, were lately found completely imbedded in the oolite quarries about a mile from Chipping Norton, near Chapel House.

A paper was also read, entitled "Observations, &c. on a Walk from Exeter to Bridport." Mr. Woods in this communication describes the nature of the soil in the neighbourhood of Exeter, and the strata exhibited in the cliffs and on the sea-shore between that city and the east side of Bridport harbour.

* See our present Number, p. 55.

NEW

NEW INSTITUTION FOR THE PROMOTION OF ZOOLOGY.

The foundations of nearly all the modern scientific institutions of this country have been recorded in the Philosophical Magazine; and it has also been our good fortune, in most instances, to report their successful progress, and the influence they have exerted on the general diffusion and extension of knowledge. The new Society whose formation and objects we have now to state, is one of those honourable associations, peculiar, we believe, to the present century, the more immediate purpose of which is the direct application of science to the arts of life,—the reduction to practical usefulness of the principles of knowledge acquired in the investigation of nature. This purpose, which is no doubt the greatest science can have in view,—next to the auxiliary support given to all true religion by the rightly-conducted examination of the works of the Creator,—renders these institutions particularly appropriate to the present elevation of natural science, in every department; and will give that elevation the fixity and the permanence always acquired by matters of mental contemplation, when embodied in works of practical utility. The promotion of Zoology in all its branches and relations is designed by the new association; but one of its chief objects is the improved and extended application to the uses of civilized society of the various races of animals, in every province of animated nature. The prospectus given below (which has been widely circulated), and some further particulars we have been favoured with, will acquaint our readers with the general views and the preliminary arrangements of the Society; and we shall only state in addition, that the plan was suggested, conjointly, by Sir Humphry Davy and Sir T. Stamford Raffles:—we need scarcely remark, that so noble a project is truly worthy of their distinguished reputation.

Prospectus of a Society for introducing and domesticating new Breeds or Varieties of Animals, such as Quadrupeds, Birds, or Fishes, likely to be useful in common life; and for forming a general Collection in Zoology.

Zoology, which exhibits the nature and properties of animated beings, their analogies to each other, the wonderful delicacy of their structure, and the fitness of their organs to the peculiar purposes of their existence, must be regarded, not only as an amusing and interesting study, but as a most important branch of natural theology, teaching, by the intelligent design and wonderful results of organization, the wisdom and power of the Creator. In its relation to useful and immediate œconomical purposes it is no less remarkable: the different races

of animals employed in social life, for labour, clothing, food, or amusement, are the direct objects of its contemplation; their improvement, the manner in which their number may be increased, the application of their produce, its connexion with various departments of industry and manufactures, are of great importance to man in every stage of his existence, but most so in proportion as he advances in wealth, civilization, and refinement.

It has long been a matter of deep regret to the cultivators of natural history, that we possess no great scientific establishments either for teaching or elucidating zoology, and no public menageries or collections of living animals, where their nature, properties, and habits may be studied. In almost every other part of Europe, except in the metropolis of the British empire, something of this kind exists; but, though richer than any other country in the extent and variety of our possessions, and having more facilities from our colonies, our fleets, and our varied and constant intercourse with every quarter of the globe, for collecting dead specimens and introducing living animals, we have as yet attempted little, and done almost nothing; and the student of natural history, or the philosopher who wishes to examine animated nature, has no other resource but that of visiting and profiting by the magnificent institutions of a neighbouring and a rival country. It is to be hoped that this opprobrium to our age and nation may disappear; and there can scarcely be a better moment for an undertaking of this kind than the present,—a state of profound peace, increasing prosperity, and overflowing wealth, when the public mind is prepared to employ its activity, and direct its resources to new objects and enterprises.

It is proposed to establish a Society bearing the same relation to zoology, that the Horticultural does to botany, and upon a similar principle and plan. The great objects should be the introduction of new varieties, breeds, and races of animals, for the purpose of domestication, or for stocking our farm-yards, woods, pleasure-grounds, and wastes: with the establishment of a general zoological collection, consisting of prepared specimens in the different classes and orders, so as to afford a correct view of the animal kingdom at large in as complete a series as may be practicable, and at the same time point out the analogies between the animals already domesticated, and those which are similar in character, upon which the first experiments were made.

To promote these objects, 1st, A piece of ground should be provided, with abundance of water, and variety of soil and aspect, where covers, thickets, lakes, extensive menageries,

and

and aviaries may be formed, and where such quadrupeds, birds, and fishes, as are imported by the Society, should be placed for ascertaining their uses, their power of increase or improvement.—2dly, Sufficient accommodation for the museum should be provided in the metropolis, with a suitable establishment, so conducted as to admit of its extension, on additional means being afforded.—It is presumed that a number of persons would feel disposed to encourage an institution of this kind; it is therefore proposed to make the annual subscription from each individual only two pounds, and the admission-fee three pounds. The members, of course, will have free and constant access to the collection and grounds, and might, at a reasonable price, be furnished with living specimens, or the ova of fishes and birds.

When it is considered how few amongst the immense variety of animated beings have been hitherto applied to the uses of man, and that most of those which have been domesticated or subdued belong to the early periods of society, and to the efforts of savage or uncultivated nations *, it is impossible not to hope for many new, brilliant, and useful results in the same field, by the application of the wealth, ingenuity, and varied resources of a civilized people.

It is well known, that, with respect to most of the animal tribes, domestication is a process which requires time, and that the offspring of wild animals, raised in a domestic state, are more easily tamed than their parents; and in a certain number of generations the effect is made permanent, and connected with a change, not merely in the habits, but even in the nature, of the animal. Even migration may be, in certain cases, prevented; and the wildest animals, supplied abundantly with food, lose the instinct of locomotion, their offspring acquire new habits, and a breed, fairly domesticated, is with difficulty brought back to its original state.

Should the Society flourish and succeed, it will not only be useful in common life, but would likewise promote the best and most extensive objects of the scientific history of animated nature, and offer a collection of living animals, such as never yet existed in ancient or modern times. The present menageries of Europe are devoted to objects of curiosity. Rome, at the period of her greatest splendour, brought savage monsters from every quarter of the world then known, to be shown

* We owe the peacock and common fowl to the natives of India; most of our races of cattle, and swans, geese, ducks, to the aborigines of Europe; the turkey, to the natives of America; the Guinea-fowl, to those of Africa. The pike and carp, with some other fishes, were probably introduced by the monks.

in her amphitheatres, to destroy or be destroyed, as spectacles of wonder to her citizens. It would well become Britain to offer another and a very different series of exhibitions to the population of her metropolis,—animals brought from every part of the globe, to be applied to some useful purpose as objects of scientific research, not of vulgar admiration; and upon such an institution, a Philosophy of Zoology founded, pointing out the comparative anatomy, the habits of life, the improvement, and the methods of multiplying those races of animals which are most useful to man, and thus fixing a most beautiful and important branch of knowledge on the permanent basis of direct utility.

March 1, 1825.

On Wednesday the 22d of June, a meeting of the friends to this Institution was held at the house of the Horticultural Society, in Regent-street, the Earl of Darnley in the chair; when, after the objects of the Institution had been stated by Sir Humphry Davy, and other gentlemen had addressed the meeting respecting them, the following noblemen and gentlemen were appointed a committee to promote the design.

Chairman, Sir T. Stamford Raffles; the Duke of Somerset, the Earl of Darnley, the Earl of Egremont, the Earl of Malmsbury, Viscount Gage, the Bishop of Carlisle, Lord Stanley, Sir H. Davy, P.R.S., E. Barnard, Esq. F.L.S., H. T. Colebrooke, Esq. F.R.S., Davies Gilbert, Esq. V.P.R.S., Rev. Dr. Goodenough, F.R.S., Sir E. Home, Bart. V.P.R.S., Thomas Horsfield, M.D. F.L.S., Rev. W. Kirby, F.R.S., T. A. Knight, Esq. P.H.S., T. A. Knight, jun. Esq., W. S. Macleay, Esq. M.A. F.L.S., Joseph Sabine, Esq. Sec. H. S., Baring Wall, Esq., N. A. Vigors, Esq. M.A. F.L.S.

The present list of members of the association is distinguished by the names of a large number of eminent men, in every department of society. Gentlemen who desire to become members will signify their wishes, by letter, to Mr. T. Griffiths, 21, Albemarle-street, London.

ROYAL ACADEMY OF SCIENCES OF PARIS.

April 4.—M. Brisson protested against the decision taken on the 21st of March, on the subject of his memoir, entitled "New researches relative to the integral calculus of partial differences:" he requested permission to leave the memoir with the secretary for three months, for the purpose of its being more fully examined:—The Academy granted this request, and appointed a new committee to report upon the memoir. —M. Thenard made a report on M. Colin's memoir relative

to

to the fermentation of sugar, which has since been printed.—MM. Portal and Duméril made a favourable report on the preparation of artificial anatomy presented by M. Auzoux.—M. Arago communicated a letter from Capt. Duperrey, respecting his voyage round the world: it contained a list of all the points which he had passed, and of the islands he had discovered; as well as of the principal observations in natural philosophy he had found occasion to make.

April 11.—M. Cordier besought the Academy to send some of its members to his dwelling, to examine a moving globe, representing the increasing and decreasing days, and also the eclipses of the sun and moon: it was determined to request him to give some details *vivâ voce* respecting the examination he solicited.—M. J. B. P. Pacchiarotti communicated a memoir, in Italian, entitled, "Physico-medical experiments on malignant fever."—M. Martin communicated his "Universal Dictionary of Divisions, with four tables serving for demonstration."—MM. Bussy and Lecanu deposited a sealed packet with the Academy.—M. G. St. Hilaire read a memoir, entitled, "Researches on some facts in the organization of the Gavials; and on the necessity of distinguishing them from the Crocodiles, as a distinct genus."—M. Arago communicated some observations on halos.—Dr. Pastré presented a memoir, entitled "An essay on the connexion between the medicinal properties of plants and the nature of their localities."

April 18.—M. de Pilaye submitted two works in manuscript to the judgement of the Academy: they were Essays on the Floras of the island of Terra Nova, and Bretagne, respectively.—M. de Humboldt communicated some observations made on the meteorite of Juvénas, by M. G. Rose, of Berlin.—M. Magendie, in the name of a committee, read a favourable report on M. Dupont's collections of animals and anatomical preparations.—M. Jomard read an extract from a memoir on the course of the Nile of the Negroes, or Niger, and that of the Nile of Egypt; with remarks on the elevation and temperature of the place where Dr. Oudeney died, east of the kingdom of Bournou.—M. Cauchy presented a memoir on the analogy between powers and differences, and on the integration of linear equations.—Dr. Duleau commenced reading a memoir containing observations on the development of hearing and speech in a youth born deaf and dumb.

April 25.—M. Arago presented, in the name of Capt. Duperrey, part of the manuscripts and drawings relative to that officer's voyage round the world; and a committee was appointed

pointed to report upon them.—Dr. Duleau finished the reading of his memoir.—MM. Fourier, Rossel, and Fresnel were appointed to assist at the examination at the *Ecole des Ponts et Chaussées*.—MM. Quoy and Gaymard read a memoir, entitled, "Descriptions of five new genera of Mollusca, and four new genera of Zoophytes, discovered in the expedition commanded by M. Freycinet."—M. Duveau read a memoir, entitled, "New researches on the natural history of Aphides."—MM. Bouvard and Mathieu were appointed to examine M. Cordier's moving globe.

X. *Intelligence and Miscellaneous Articles.*

NEW ARRIVAL OF ANIMALS FROM SUMATRA.

OUR readers must be already acquainted with the loss sustained by Sir T. Stamford Raffles, of the most valuable collection of Natural History ever formed in the East, in consequence of the destruction by fire of the ship Fame. They have also heard, it is probable, of the valuable collection he succeeded in forming and bringing safely home after that disastrous event. We are now happy to state that Sir Stamford has just received from his correspondents at Bencoolen a considerable addition to his collection, including the *Ursus malayanus*, and the male and female Javanese Peacocks, all living; which have been placed, for the present, in the menagerie at the Tower. Also the skin and skeleton of a female Orang-Outang, which animal has been recently discovered in Sumatra; together with an extensive series of quadrupeds, birds, reptiles, and insects.

ACCOUNT OF THE MUMMY DISSECTED BY DR. GRANVILLE.

The following is a correct abstract of Dr. Granville's "Monograph on Egyptian Mummies, with Observations on the Art of Embalming among the Ancient Egyptians," lately read before the Royal Society.

The principal object of this paper was to describe a mummy purchased at Gournou, in Upper Egypt, and presented to the author by Sir A. Edmonstone, bart. It was in a single case, of the usual form, and covered with cerecloth and bandages very neatly and dexterously applied, exhibiting almost every bandage and compress employed in modern surgery, and among which both cotton and linen were recognised:—these, to the amount of 28lbs. avoirdupois in weight, having been removed, the body proved to be that of a female. The abdominal integuments were remarkably wrinkled; and the whole surface was of a dark-brown colour and dry, but in many places

places soft to the touch, and, with the exception of a few parts, entirely deprived of cuticle. The height of the mummy, from the vertex of the head to the inferior surface of the calcaneum, was 5 feet $\frac{7}{10}$ inch; and the principal dimensions of several parts correspond with those which are usually considered as giving rise to the utmost perfection of the female form in the European race;—thus these dimensions are precisely those assigned by Camper and Winkelmann to that celebrated statue the Medicean Venus;—and no *trait* of Ethiopian character was discernible in the form of the cranium: all which, Dr. Granville observed, supports Cuvier's opinion respecting the Caucasian origin of the Egyptians.

Dr. Granville then proceeded to a brief summary of the present state of our information respecting Egyptian mummies, attributing its scantiness and imperfection to the rarity of perfect specimens, nearly all the mummies hitherto described presenting little else than imperfect skeletons, sometimes covered by the dry skin, enveloped in bandages.

In proceeding to examine and dissect the present specimen, which was effected in the presence of several medical and scientific friends of the author, the integuments and muscles of the abdomen were first removed, and the contents of that cavity carefully inspected: they consisted of a portion of the stomach adhering to the diaphragm, the spleen attached to the super-renal capsule of the left kidney, and the left kidney itself, with the ureter descending into the bladder, which, with the uterus and its appendages, were observed *in situ*, the latter exhibiting marks of disease. Fragments only of the intestinal canal were discoverable; and there were a few lumps of resin, and of a mixture of clay and bitumen, and a few pieces of myrrh. The right kidney, the liver, and the minor glands were missing; but the gall-bladder was detected among the loose fragments of membranes and other soft parts, together with remains of its own ducts. The soft parts of the pelvis were then particularly examined, and the perfect condition of the muscles, membranes, and ligaments particularly noted. The cavity of the thorax was next examined, by detaching the diaphragm, to which part of the pericardium adhered; and the heart, in a very contracted state, was afterwards found suspended by its vessels and attached to the lungs, which adhered to the ribs.

Upon the examination of the cranium, it was evident that the brain had been removed through the nostrils, from the lacerated condition of the inner nasal bones: the eyes appeared not to have been disturbed; the tongue was entire, and the teeth were white and perfect.

Dr.

Dr. Granville next proceeded to draw some conclusions as to the age at which this mummified female died, and respecting the disease which destroyed her. The bones of the ilium exhibit that peculiar thinness of their osseous plates which shows the individual to have exceeded her fortieth year, and to have borne children; and as there are no characters of age or of decrepitude about the skeleton, the author considers her to have been about fifty. The ovarium and broad ligament of the right side were enveloped in a mass of diseased structure, while the fallopian tube of the same side was sound; but the uterus itself was larger than natural, and the remains of a sac were found connected with the left ovarium,—all which, in conjunction with the appearance of the abdominal integuments, leave no doubt of ovarian dropsy having been the disease under which the individual suffered.—Judging from the excavation out of which the mummy was taken, and according to the best authorities of the present day on Egyptian Antiquities, the period at which the woman lived must have been about three thousand years ago.

The author concludes this communication with some observations respecting the method of embalming practised by the ancient Egyptians, and the nature of the substances employed in the process; from the details of which, in conjunction with the results of his own researches and experiments, as well synthetical as analytical, he draws the conclusions following: That the abdominal viscera were more or less perfectly abstracted either through an incision on one side of the abdomen, or, as in the present mummy, through the anus. The thoracic cavity was not disturbed. That the contents of the cranium were removed,—sometimes through the nostrils, and in others through one of the orbits. The body was then probably covered with quicklime, to facilitate the removal of the cuticle, the scalp and nails being however left untouched; after which it was immersed in a melted mixture of bees'-wax, resin, and bitumen, until thoroughly penetrated; and, ultimately, subjected to a tanning liquor, probably made with the saline water of the neighbouring natron lakes. The bandages were then applied, with the occasional interposition of melted resin, or wax and resin, the lumps of resin, myrrh, &c., having been previously placed in the abdomen.

In order fully to establish these conclusions respecting the mummifying process, Dr. Granville had prepared several imitative mummies by its means; some of which bore the closest resemblance to the Egyptian, and had withstood putrefaction for upwards of three years, though exposed to the vicissitudes of a variable climate without any covering, or other precautionary

cautionary measure. None of the substances used appear to be sufficient, either singly or conjointly, without the wax, to preserve the body, or convert it into a perfect mummy: and one of the *nates*, having been wholly deprived of the wax by ebullition and maceration, looked no longer like its mummified fellow, but resembled a preparation of a recent specimen of that part, and soon began to putrefy. After the readings of the paper, Dr. Granville exhibited the dissected mummy and its various parts, together with the bandages with which it had been invested, drawings of its outer case, &c., and his own imitative preparations, in the Society's library; thus illustrating the details of his communication.

WHITE COPPER.

M. Frick, a German chemist, has formed several alloys in imitation of white copper, or the *Pakfong* of the Chinese. A mixture in the following proportions—

Copper	41·75
Nickel	32·25
Zinc	26·00

composed a grayish alloy, very little malleable when cold, not at all when heated; flattening with difficulty. Another according to this formula—

Copper	50·00
Zinc	31·25
Nickel	18·75

produced a white metal susceptible of a beautiful polish, easily flattened, malleable when cold, unalterable by the atmosphere, and sonorous like silver. A third alloy, formed as follows—

Copper	53·39
Zinc	29·13
Nickel	17·48

approached still nearer to silver in colour and sound. It was harder than that metal, very tenacious, but also exceedingly ductile. Its sp. gr. was at 15°,4 of Reaum. 8·556.—*Bullet. Univers.**

MR. FARADAY ON NEW COMPOUNDS OF CARBON AND HYDROGEN.

A paper was communicated to the Royal Society, June 16, On some new compounds of carbon and hydrogen, and on certain other products obtained during the decomposition of oil by heat, by Mr. M. Faraday, F.R.S.

The experiments, of which the results are detailed in this paper, were made principally on the fluid, which is found to be deposited in considerable quantity, when oil gas is com-

* See Phil. Mag. vol. lxiii. p. 119.

pressed. This fluid, as obtained at the works of the Portable Oil Gas Company, is colourless, of a specific gravity less than that of water, insoluble in water except in very minute quantities, soluble in alcohol, ether, oils, &c., and combustible, burning with a dense flame. It is strikingly distinguished from the oil from which it originated, by not being acted upon to any extent by solutions of the alkalies.

Part of this fluid is very volatile, causing the appearance of ebullition at temperatures of 50° or 60°. Other parts are more fixed, requiring even 250° or above for ebullition. By repeated distillations, a series of products were obtained from the most to the least volatile, the most abundant being such as occurred from 170° to 200°. On subjecting these, after numerous rectifications, to a low temperature, it was found that some of them concreted into a crystalline mass; and, ultimately, a substance was obtained from them, principally by pressure at low temperatures, which upon examination proved to be a new compound of carbon and hydrogen. At common temperatures it appears as a colourless transparent liquid, of specific gravity 0·85 at 60°, having the general odour of oil gas. Below 42° it is a solid body, forming dendritical transparent crystals, and contracting much during its congelation. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf sugar. It evaporates entirely in the air: when raised to 186° it boils, furnishing a vapour, which has a specific gravity of 40 nearly, compared to hydrogen as 1. At a higher temperature the vapour is decomposed, depositing carbon. The substance is combustible, liberating charcoal, if oxygen be not abundantly present. Potassium exerts no action upon it below 186°.

This substance was analysed by being passed over red-hot oxide of copper, and by detonation of its vapour with oxygen. The results obtained were, that it consists of

2 proportionals of carbon	12
1 ——————— hydrogen	1
	13

and that, in the state of vapour, six proportionals of carbon and three of hydrogen are present to form 1 volume, which is consequently of the specific gravity of 39, hydrogen being 1. It is named in the paper *Bi-carburet of hydrogen.*

Experimenting with the most volatile portions of the liquid, a product was obtained, which, though gaseous at common temperatures, condensed into a liquid at 0°. This was found to be very constant in composition and properties. It was

very

very combustible. It had a specific gravity of 27 or 28 as a gas; as a liquid that of 0·627, being the lightest substance, not a gas or vapour, known. When analysed, it was found to consist of one proportional of carbon 6, and one of hydrogen 1, as is the case with olefiant gas; but these are so combined and condensed, as to occupy only one half the volume they do in that substance. A volume therefore of the gas contains four proportionals of carbon 24, and four of hydrogen 4 = 28, which is its specific gravity.

Beside the remarkable difference thus established between this substance and olefiant gas, it is also distinguished by the action of chlorine, which forms with it a fluid body having a sweet taste, and resembling hydro-chloride of carbon; but from which a chloride of carbon cannot be obtained by the further action of chlorine and light.

The other products from the original fluid do not present any characters so definite as the above substances; at the same time they appear to be very constant, boiling uniformly at one temperature. They cannot be separated by distillation into more and less volatile parts, so as to afford means of reducing their number to two or three particular bodies. They have the general properties of the original fluid, and, with the other products, are all peculiarly acted upon by sulphuric acid, offering phænomena, in the investigation of which the author is at present engaged.

With reference to the presence of these substances in the state of vapour in oil and coal gas, the means of ascertaining it, and the quantity, are pointed out, in the peculiar action of sulphuric acid, causing their perfect condensation, and in the solvent powers over them possessed by fixed and volatile oils, &c., the requisite precautions for their proper application being described. Oil gas was found to be saturated with many of these vapours. Coal gas also contained a portion of them.

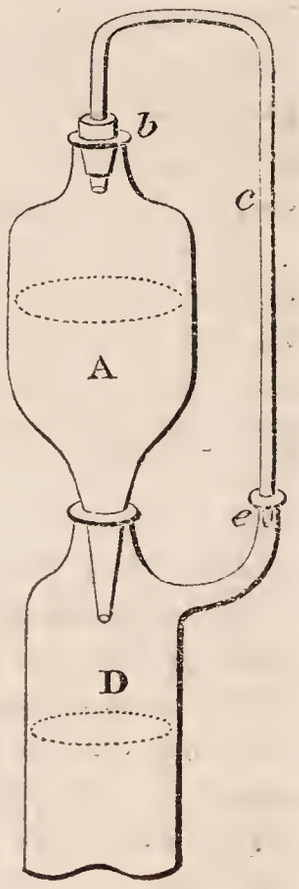
The paper concluded with a short reference to the probable uses of the fluid, as originally obtained. If put into gas burning with a blue flame, it makes it produce a bright white flame. It is an excellent solvent of caoutchouc: it will answer all the purposes to which essential oils are applied as solvents; and, having applied that portion of it, which though at common temperatures a liquid at a pressure of 2 or 3 atmospheres, is a gas under any diminished pressure, as fuel to a lamp; the author has shown the possibility of such an application, if at any time such knowledge and command of the decomposition of oil or coal by heat should be obtained, as would enable us to furnish the substance in abundance.—*Journal of Science.*

MR. DONOVAN'S APPARATUS FOR FILTERING OUT OF CONTACT WITH THE ATMOSPHERE.

Where alkaline solutions in their caustic state are to be filtered, and where it is required that the solution, after filtration, should still be in the caustic state, it becomes a matter of some difficulty to exclude the atmosphere in such a way as to prevent the absorption of carbonic acid from it by the alkali.

Thus in the process for preparing pure potash, when the carbonate of potash, the hydrate of lime, and the water, have been exposed to each other's action, during a sufficient period, it is found difficult to separate the lime by the ordinary process of filtration. The mass is of so absorbent and pasty a nature, that the liquor passes through the filter very slowly and with great difficulty; hence it is generally carbonated in its passage, and the contrivances commonly resorted to, of covering the mouth of the vessel, &c. succeed very imperfectly.

Difficulties of this kind compelled me to contrive some means of conducting filtrations out of contact with the atmosphere; and I found that the end can be attained by a very simple apparatus.



The instrument consists of two glass vessels, the upper one A has a neck at *b*, which contains a tight cork so perforated as to admit one end of the glass tube *c*. The other end of the vessel A terminates in a funnel pipe, which fits into one of the necks of the under vessel D, by grinding, or luting, or by a tight cork. The vessel D has also another neck *e*, which receives the other end of the tube *c*, the juncture being secured by a perforated cork, or by luting. The throat of the funnel pipe is obstructed by a bit of coarse linen loosely rolled up, and not pressed down into the pipe. The alkaline solution, containing the lime, is then to be poured in through the mouth at *b*, the cork and tube having been removed; and the first droppings are to be allowed to run to waste, and are not to be received into the under vessel D. The parts of the apparatus are now to be joined together, and the filtration may proceed at the slowest rate without the possibility of any absorption of carbonic acid by the alkali.

It is scarcely necessary to explain the manner in which this simple apparatus acts. It is obvious that no fluid can drop

drop out of the upper vessel unless an equal volume of air can enter it; and that no fluid can enter the lower vessel unless an equal volume of air can escape from it. Both of these conditions are fulfilled by means of the connecting tube *c*: for every drop of liquor that falls into the lower vessel expels its own bulk of air; and this air having no other means of escape passes along the connecting tube to the upper vessel, where exactly that bulk of air is required to compensate the vacuum that would have otherwise been so far formed, by the loss of the drop of liquid which had fallen through the funnel pipe. Thus the transfer of the liquor takes place from the upper to the lower vessel, as also the compensating transfer of air from the lower to the upper vessel; and this goes on to the end, the alkali being only exposed to the same portion of air during any period of time, be it ever so long, that may be required for the completion of the process.

It is to be observed that this apparatus should be made of green glass, it being much less acted on by fixed alkalies than white. A white glass bottle containing solution of caustic potash will often be cracked by it in every direction, and in a singular manner.

This apparatus is extensive in its utility: it answers for the filtration of any liquid where either the carbonic acid or the moisture of the atmosphere would be injurious. It is also well adapted for the filtration of volatiles, as alcohol, ethers, ammoniacal liquors, &c., the vapours being thus effectually confined. And by substituting a stratum of coarsely powdered flints in the usual manner, instead of the roll of linen, we may filter corrosive acids, which would be weakened by access of air.—*Dublin Phil. Journ.*

ON CADMIUM. BY JAMES APJOHN, M.B.

In examining a slag produced in the smelting of galena, I have succeeded in detecting the presence of cadmium, a metal hitherto unknown in this country [Ireland]. Prof. Stromeyer, of Göttingen, was the first who described this substance, which was found by him in zinc brought from Silesia. Its principal properties and combinations were fully explained by this distinguished chemist. It was subsequently found by Dr. Clarke of Cambridge to exist in minute quantity, in some of the spelter ores of Mendip and Cornwall.

Of the slag in which I have discovered it, it constitutes, by a single trial, 5·05 grs. per cent. From a subsequent experiment, however, I am disposed to think that the average proportion in which it enters into the composition of the slag is not so high.

high. The following is an outline of the process by which it was isolated.

Upon the slag, when pulverized, nitric acid was digested, until it ceased to exert any further solvent energies. When filtered, the solution was nearly neutralized, and sulphate of soda added, while any precipitate formed. The precipitate, which was sulphate of lead, was collected on a filter. To the filtered liquor caustic ammonia was added, which at first threw down a copious precipitate; the greater part of which was subsequently re-dissolved, by an excess of the precipitant. The insoluble portion, which was oxide of iron, was separated by a filter. The ammoniacal solution was submitted, on a sand bath, to evaporation; by which the oxides dissolved in the ammonia were gradually thrown down. These were then dissolved in muriatic acid, and to the solution carbonate of ammonia was added. A copious precipitate at first occurred, which was considerably reduced by adding the carbonate of ammonia in excess, in consequence of the property this salt possesses of dissolving the oxide of zinc, which was present in considerable quantity. The undissolved residuum, when well washed, was re-dissolved in muriatic acid, and then introduced into a platinum capsule, containing a slip of zinc. When examined next day, the bottom of the capsule was found lined with a dark lead-coloured metallic coating, which adhered so firmly as to admit of being repeatedly washed with distilled water without being displaced. This experiment, as is well known to chemists, was first made by Dr. Wollaston. It was again dissolved in a drop of muriatic acid, and a small quantity of the solution put in a test tube, into which a little hydrosulphuret of ammonia was subsequently introduced. This threw down an orange precipitate. To another portion of the solution caustic potash was added in excess; the white precipitate at first formed was not re-dissolved.

The identity then of this metal with cadmium is fully established—by the solubility of its oxide in ammonia; its insolubility in carbonate of ammonia and caustic potash; the colour of the precipitate with an alkaline hydrosulphuret; and the peculiar appearance of the metal when precipitated by zinc upon the platinum capsule. I may also observe, that a minute portion of the oxide, examined by the blowpipe, presented the well known characteristic properties of cadmium.—*Dublin Phil. Journ.*

LIST OF NEW PATENTS.

To J. J. Saintmare, of Belmont Distillery, Wandsworth Road, Surrey, distiller, for improvements in distilling.—Dated 28th June 1825.—6 months to enrol specification.

To

To David Redmund, of Old-street Road, Middlesex, engineer, for improvements in building ships, houses, &c.—28th June.—6 months.

To George Thompson, of Wolverhampton, for improvements in the construction of saddles.—28th June.—6 months.

To John Heathcoat, of Tiverton, lace-manufacturer, for improvements in manufacturing thrown silk.—6th July.—6 months.

To William Heycock, cloth-manufacturer, of Leeds, for improvements in machinery for dressing cloth.—8th July.—6 months.

To John Biddle, of Dormington, Salop, glass-manufacturer, for his machinery for making, repairing, and cleansing roads, paths, &c.—8th July.—6 months.

To Lieut. Molyneaux Shieldham, of Brampton Hall, Wrangford, Suffolk, for improvements in setting, working, reefing, and furling the sails of vessels.—8th July.—2 months.

To William Furnival and John Craig, both of Anderton, Cheshire, salt-manufacterers, for improvements in the manufacturing of salt.—8th July.—6 months.

To John Day and Samuel Hall, of Nottingham, lace-manufacturers, for their improvement on a pusher twist or bobbin-net machine.—8th July.——2 months.

To Walter Hancock, of King-street, Northampton-square, Middlesex, for improvements in the making of pipes for the passage of fluids.—16th July.—6 months.

To William and Henry Hurst, of Leeds, for improvements in scribbling and carding sheep's wool.—16th July.—6 months.

To Henry Hurst, manufacturer, and George Bradley, machine-maker, both of Leeds, for improvements in looms for woollen cloths.—16th July.—6 months.

To Thomas Wolrich Stansfeld, merchant, William Prichard, civil engineer, and Samuel Wilkinson, merchant, of Leeds, for improvements in looms and in the implements connected therewith.—16th July.—6 mon.

To Thomas —— of Devizes, saddler, for improvements in collars for horses and other animals.—16th July.—2 months.

To Marc Isambard Brunel, of Bridge-street, Blackfriars, London, for mechanical arrangements for obtaining powers from fluids, and for applying the same to various useful purposes.—16th July.—6 months.

To Thomas Sitlinton, of Stanley Mills, Gloucestershire, engineer, for improvements in machinery for shearing or cropping woollen or other cloths.—16th July.—6 months.

To Joseph Farey, of Lincoln's Inn Fields, Middlesex, civil engineer, for improvements in lamps.—16th July.—6 months.

To Thomas Robinson Williams, of New Norfolk-street, Strand, Middlesex, for an improved lancet.—16th July.—6 months.

To Lieut. Thomas Cook, of Upper Sussex-place, Kent Road, Surrey, for improvements in the construction of carriages and harness, for the greater safety of persons riding.—16th July.—6 months.

To Joseph Cheseborough, dyer, of Manchester, for a method of conducting to and winding upon spools, or bobbins, rovings of cotton, flax, wool, or other fibrous substances. Communicated from abroad.—16th July.—6 months.

To William Hurst, and Joseph Carter, cotton spinner, of Leeds, for an apparatus for giving a new motion to mules or billies.—16th July.—6 mon.

To John Palmer De la Fons, of George-street, Hanover-square, dentist, for improvements in extracting and fixing teeth.—16th July.—6 months.

To Jonathan Downton, of Blackwall, Middlesex, shipwright, for improvements in machines or pumps.—19th July.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. CARY in London, and Mr. VEALL at Boston.*

Gosport, at half-past Eight o'Clock, A.M.								Clouds.							Height of Barometer, in Inches, &c.		Thermometer. London.			Thermometer. Boston.	Rain.		Weather.		
Days of Month, 1825.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	Bost. 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
June 26	29·86	61	50·80	54	W.	...	0·090	1	...	1	...	1	1	1	29·85	29·35	54	65	55	57	...	0·17	Showery	Fine[25th rain *p.m.*]	SW.
27	29·84	60	.....	54	SW.	0·40	·010	1	1	1	...	1	1	1	29·88	29·45	55	60	54	57	...	·31	Showery	Fine, rain *a.m.*	SW.
28	29·80	61	.....	57	S.	...	·060	1	1	1	...	1	1	1	29·74	29·47	54	60	55	57·5	...	...	Showery	Cloudy, rain *p.m.*	S.
29	29·78	62	.....	57	NW.	...	·035	1	...	1	...	...	1	1	29·79	29·37	55	67	57	56	...	·22	Cloudy	Cloudy	SW.
○ 30	29·78	64	.....	64	SW.	·40	·065	1	1	1	...	1	1	1	29·76	29·30	56	65	56	60	...	...	Cloudy	Cloudy, rain *p.m.*	SW.
July 1	29·83	64	51·00	55	W.	...	·010	1	1	1	...	1	1	1	29·86	29·38	56	67	52	61	...	·02	Cloudy	Fine, rain *p.m.*	SW.
2	30·17	60	.....	57	NE.	...	.....	...	...	...	...	1	1	...	30·20	29·80	55	66	58	57	...	·13	Fair	Fine	W.
3	30·23	61	.....	56	NW.	·95	.....	1	...	1	...	1	...	...	30·21	29·80	57	74	66	62	0·12	...	Fair	Fine	SW.
4	30·16	66	.....	55	NW.	·25	·010	1	1	1	...	1	1	1	30·22	29·72	64	71	63	64·5	...	...	Fair	Cloudy	W.
5	30·34	64	.....	54	NW.	...	.....	1	...	1	...	1	1	...	30·33	29·87	60	67	62	62·5	...	...	Cloudy	Fine	NW.
6	30·18	65	.....	54	NW.	·50	·060	...	...	1	...	1	1	1	30·14	29·73	60	62	50	59·5	...	...	Cloudy	Cloudy	Calm.
7	30·07	59	51·10	59	NE.	...	.....	...	1	1	...	1	1	1	30·06	29·72	51	61	54	58	...	...	Cloudy	Cloudy	NW.
☾ 8	30·03	61	.....	60	N.	...	.....	...	1	...	...	1	1	...	30·07	29·70	55	64	56	57	...	...	Cloudy	Cloudy	NW.
9	30·04	58	.....	60	N.	·45	.....	1	1	1	...	1	1	...	30·05	29·72	54	67	57	58·5	...	...	Fair	Cloudy	W.
10	30·02	64	.....	58	NE.	...	.....	1	...	1	...	1	1	...	30·01	29·63	60	68	61	57	·00	...	Cloudy	Cloudy [and even^{g}.	N.
11	30·00	65	.....	57	E.	...	.....	1	1	1	...	1	1	...	30·01	29·63	63	75	66	59	...	...	Fair	Cloudy, fog^{y}. morn^{g}.	NE.
12	30·03	66	51·50	56	S.	·70	.....	1	1	1	...	1	...	...	30·04	29·60	66	77	67	66	...	...	Fair	Fine, rain *p.m.*	NE.
13	30·13	72	.....	55	W.	·40	.....	1	1	1	...	1	...	...	30·22	29·58	66	77	67	68	...	·09	Fair	Fine	S.
14	30·18	68	.....	56	SE.	·40	.....	1	...	1	...	...	...	...	30·16	29·50	60	79	70	73	...	...	Fair	Fine, Ther. 5 *p.m.* 80	S.
● 15	30·00	73	.....	51	SE.	·30	·100	1	1	1	1	1	1	1	30·05	29·45	81	84	72	78	...	...	Fair	Do. 1 *p.m.* 89, rain	E.
16	30·18	75	.....	56	N.	·42	.....	1	...	1	...	1	...	...	30·25	29·58	70	83	68	71·5	...	·01	Fair	Fine, Ther. 3 *p.m.* 82	SW.
17	30·27	76	52·00	50	NE.	·40	.....	...	...	...	...	...	...	...	30·28	29·70	70	83	70	72	·00	...	Fair	Fine, Ther. 3 *p.m.* 81	S.
18	30·17	79	.....	51	NE.	·45	.....	...	1	...	...	1	...	...	30·25	29·60	70	86	72	76	...	...	Fair	Fine, ext. heat 81	NE.
19	30·18	80	.....	50	NE.	·65	.....	1	1	...	...	1	1	...	30·23	29·60	72	89	69	78	...	...	Fair	Fine, ext. heat 85	NE.
20	30·24	75	.....	46	NE.	·60	.....	1	...	...	...	...	...	...	30·32	29·76	67	80	64	66	...	...	Fair	Cloudy	N.
21	30·20	68	.....	50	E.	·40	.....	...	...	1	...	...	1	...	30·26	29·75	62	69	55	63·5	...	...	Fair	Cloudy	NW.
☽ 22	30·17	62	.....	50	NE.	·30	.....	1	...	1	...	1	...	...	30·14	29·74	55	69	60	61·5	...	...	Fair	Fine	SW.
23	30·00	63	.....	49	NE.	·25	.....	...	...	1	...	1	1	...	30·03	29·60	55	65	56	58	...	...	Fair	Cloudy	W.
24	30·08	60	.....	50	NE.	·30	.....	...	...	...	...	1	1	...	30·14	29·70	56	68	57	60	·00	·02	Fair	Do. rain *a.m.* early	W.
25	30·30	61	52·20	48	NE.	·35	.....	1	1	1	...	1	1	...	30·31	29·85	57	66	60	60	...	...	Fair	Cloudy	SW.
Aver. :	30·075	65·77	51·47	54·3		8·87	0·440	21	16	23	1	25	21	10	30.10	29·62	61	71	61	63·1	0·12	0·97			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *AUGUST* 1825.

XI. *On the Constitution of the Atmosphere.* *By* J. Ivory, *Esq.* *M.A. F.R.S.*

1. IT is well known that, as we ascend in the atmosphere, a continually increasing degree of cold is found to prevail. There are no doubt irregularities and exceptions. Particularly, in small heights near the earth's surface, the cold is sometimes greater below than above. But in general, and at great heights beyond the immediate influence of the earth, the fact cannot be contested.

This peculiar disposition of heat in the atmosphere has not been much inquired into. The account of it proposed by Dalton is the only one which seems deserving of notice. According to that philosopher, a given mass of air, whatever be its place in the atmosphere, retains always the same absolute quantity of heat. When it ascends and becomes rarer by diminished pressure, the cold produced, or the heat which disappears from the thermometer, enters into combination with the dilated air. On the other hand, when it descends, the condensation produced by the greater pressure causes it to give out part of the heat combined with it in a latent form, which becomes sensible to the thermometer. If we conceive a perpendicular column of air extending from the earth's surface to the top of the atmosphere to be divided into equal masses, the respective portions will successively occupy larger spaces; and, according to the principle of Dalton, we shall have a gradation of heat decreasing upwards as observation requires.

In all our inquiries into the properties of the atmosphere, it is supposed that the height of a mass of air, its pressure, its density, and its temperature are all intimately connected. A change in one of these four things is necessarily attended with a variation of all the rest. When the height increases, the pressure, the density, and the temperature decrease: and a mass of air cannot remove to a lower place, without receiving an augmentation of pressure, density, and temperature. As

there are no circumstances connected with these facts, except what we have mentioned, it seems to be impossible to account for the heat which disappears from the thermometer in the one case, and again becomes sensible in the other, unless we suppose that it enters into combination in a latent form with the dilated air, and is again evolved when the same air recovers its former volume. So far we may allow that Dalton has reasoned rightly with regard to the decrease of heat in the atmosphere; but, as we may form innumerable hypotheses connecting the variations of temperature with the changes of density, the principal object of research remains in a manner untouched.

Professor Leslie has adverted to this subject in the article *Climate* in the Supplement to the *Encyclopædia Britannica*. He explicitly adopts the theory of Dalton, although he has not mentioned the author from whom he borrowed it. The demonstration he has attempted is certainly not much to the purpose*. In whatever way we account for the constitution of the atmosphere, we must conceive that there is a mean state about which it makes small oscillations on opposite sides. In a stratum of air at a given height above the earth's surface, the heating and cooling causes will balance one another in a length of time, and the mean temperature will be invariable. But this gives us no information concerning the law of temperature in passing from one stratum to another. While the mean temperature of each stratum remains constant, the gradation with respect to the different strata, may be infinitely varied. The theory of Dalton has nothing to do with increments and decrements of heat received at different times, and finally compensating one another; it is founded on a property inherent in air, according to which the temperature of any given mass is determined by the dilatations and condensations it is made to undergo.

2. Leaving general arguments, let us inquire what light will be thrown upon this subject by the knowledge of the condition of the atmosphere that has actually been obtained. And here all our information is derived from one source, namely, the measurement of heights by the barometer. Fortunately great pains have been taken to perfect this method, which has been extensively applied by many careful observers. We are thus enabled to compare the temperatures and pressures determined by actual observation at many different elevations.

Ramond, in his work on the barometrical formula, has collected 42 different measurements, for the express purpose of

* *Suppl. Encyclopædia Britannica,* vol. iii. p. 185.

ascertaining

ascertaining the rate of the decrease of temperature. In three of these instances the temperature decreases with extreme slowness, and in one with unusual rapidity. Setting aside these four cases, the irregularity of which may be ascribed to local peculiarities, the remaining 38, extending through a large scale of temperature, and embracing every variety of altitude, give a mean ascent of 164·7 metres, or 90 English fathoms, for one degree of depression of the centigrade thermometer. The greatest altitude included in the 38 measurements is Gay-Lussac's ascent,—nearly 7600 yards; and in this particular case, the height for one degree of depression comes out equal to 95 fathoms. The only general inference that can be drawn from a comparison of all the experiments is this: That the decrease of heat is nearly proportional to the increase of altitude, the rate being about one degree of the centigrade thermometer for every 90 fathoms of ascent. But this law must be understood as belonging to a mean state of the atmosphere, and as occasionally liable to great irregularities, more especially in the vicinity of the earth's surface.

Now if we adopt the law of an equable decrease of heat in proportion to the altitude, the conditions required for the equilibrium of elastic fluids will enable us to determine the gradation of heat and pressure in the atmosphere. For the investigation of this point, I shall refer to a paper on the astronomical refractions printed in the Philosophical Transactions for 1823. Supposing that, at the earth's surface, the pressure of the air, its density, and the function for temperature, are each represented by unit, it is shown in the paper cited, that when the density is reduced to $1-\omega$ by ascending in the atmosphere, the pressure or the elasticity of the air will be equal to $(1-\omega)^{\frac{5}{4}}$, and the function for temperature to $(1-\omega)^{\frac{1}{4}}$. Applying these formulæ to the measurements of Ramond, they are found to give very accurate results. And if we seek the expression of the height at which the diminished density $1-\omega$ will take place, we fall upon the usual formula for measuring heights with the barometer. The actual state of the atmosphere is therefore as well represented as the nature of the inquiry seems to permit.

Professor Leslie has investigated a formula for the increased capacity of rarefied air, or for computing the heat disengaged or absorbed when a mass of air undergoes a given condensation or rarefaction. In the whole of experimental science, perhaps, no other instance will be found in which it is so difficult to form exact and adequate ideas of the train of investigation, and the degree of precision that must be attached to

the result. The process seems to be a specimen of mental ingenuity very slightly supported by facts, and corrected at every step by calculations almost purely conjectural. But no experimental research could entirely fail in such able hands; and although the Professor has not been successful in the principal object he had in view, yet in another point he has added to science, or at least to the rules for calculation. His formula certainly does not accomplish what is the express purpose of his research: it does not enable us to compute the heat evolved when air is suddenly condensed, or absorbed when it is suddenly rarefied. Thus it completely fails when applied to correct Newton's calculation of the velocity of sound, although it is now known that, in this instance, theory is sufficiently reconciled with experiment, when a proper estimate is made of the increased elasticity of air arising from the heat disengaged by compression. What the formula does enable us to compute, is the temperature at a height in the atmosphere where the given elasticity prevails. According to Dalton this temperature ought to be equal to the cold produced by an equal rarefaction at the earth's surface. But here the hypothesis of the philosopher of Manchester is at variance with nature; for air rarefied at the earth's surface is colder, by one part in four, than air of the same elasticity in the atmosphere.

Nothing certainly can be more reprehensible than to find fault with the labours of others without explaining fully, and proving clearly, the grounds on which the objections are made. The practice of which I have ventured to disapprove is not however uncommon in the present times; and that I may not furnish a new instance of it, I shall lay before the reader the reasons of what has been advanced respecting Professor Leslie's formula. Let l denote the length of the homogeneous atmosphere, equal to 4500 fathoms at the mean temperature of 50° of Fahrenheit; x any height in the atmosphere; and θ the relative pressure or elasticity of the air at that height: then c being the base of the hyperbolic logarithms, we obtain by what is usually taught in elementary treatises on the supposition of a uniform temperature in the atmosphere,

$$\theta = c^{-\frac{x}{l}}.$$

Consequently,

$$\frac{1}{\theta} = c^{\frac{x}{l}};$$

and

$$\frac{1}{\theta} - \theta = c^{\frac{x}{l}} - c^{-\frac{x}{l}};$$

by

by expanding the exponentials,

$$\frac{1}{\theta} - \theta = \frac{2x}{l} \times \left\{1 + \frac{1}{6} \cdot \frac{x^2}{l^2} + \frac{1}{120} \cdot \frac{x^4}{l^4} + \&c.\right\}.$$

Now l is about five miles; and the greatest height hitherto ascended in the atmosphere is only $4\frac{1}{4}$ miles; therefore, for all accessible heights, we may suppose that the series in the last expression coincides with its first term: thus we have

$$\frac{1}{\theta} - \theta = \frac{2x}{l}.$$

Let τ denote the difference of the temperatures at the height x and the surface of the earth in degrees of the centigrade thermometer; then, allowing 90 fathoms of ascent for every degree, we get $x = \tau \times 90$: consequently,

$$\frac{1}{\theta} - \theta = \frac{180}{l} \times \tau = \frac{\tau}{25};$$

and

$$\tau = 25\left(\frac{1}{\theta} - \theta\right).$$

This is the Professor's formula, which after all appears to be no more than a deduction from the usual barometrical theory, combined with the hypothesis of an equable decrease of heat in proportion to the altitude. It is not so much a physical discovery as it is an algebraic transformation of the expressions of the pressure and temperature that prevail in the atmosphere. In whatever form we obtain the quantities mentioned, the formula is still readily deduced as a corollary. Thus, if we use the expressions of the pressure and temperature mentioned a little above, we have

$$\theta = (1 - \omega)^{\frac{5}{4}},$$

$$1 - \alpha\tau = (1 - \omega)^{\frac{1}{4}};$$

where α stands for $\frac{3}{800}$, or the dilatation for one degree of the centigrade thermometer. Now, rejecting the square and higher powers of ω, we get

$$\frac{1}{\theta} - \theta = \frac{5}{2} \cdot \omega$$

$$\alpha\tau = \frac{\omega}{4}:$$

and by exterminating ω,

$$\tau = 26^{\circ}{\cdot}7\left(\frac{1}{\theta} - \theta\right);$$

and this formula will probably be found more exact in practice than the other.

Among the inferences drawn by Mr. Leslie from his formula, there is one it will be proper to mention here. He makes

makes the heat of the atmosphere decrease more rapidly than the altitude increases. It is difficult to determine this point in a satisfactory manner by a comparison of actual measurements, if we confine our attention to small heights above the earth's surface. The irregularity in such cases is so great, that by a proper choice of instances the result may be made to favour either an accelerating or a retarding scale of heat*. But great altitudes lead unequivocally either to an equable gradation, or to one continually decreasing in its rate. Thus in the instances collected by Ramond, the mean height for one degree of depression is upon the whole 90 fathoms; but in the great altitude ascended by Gay-Lussac, taken by itself, the like quantity is 95 fathoms, which is incompatible with an accelerated rate of decrease. The opinion that the decrements of heat are more rapid the higher we ascend in the atmosphere,—which Professor Leslie has adopted, and which he has illustrated with a profusion of elementary geometry,—was no doubt suggested by his formula, from which it follows as a necessary consequence. But the experimental investigation of the formula itself is merely an ingenious chimera, which leads to an inexact result. The real foundation of it is the barometrical theory, to which it makes no addition, and the conclusions of which it cannot, with any pretension to right reasoning, be employed to overturn.

Dr. Young has followed Professor Leslie in adopting an accelerated decrease of heat, which seems to favour some of his peculiar notions about the astronomical refractions. He quotes the authority of Humboldt in support of his opinion. As the quotation is general, it might not be easy to find the particular passage alluded to; but in Ramond's work I find the following words relating to the same point.

"Au voisinage de la terre, le decroissement de la chaleur est ordinairement d'une lenteur extrême, et quelquefois d'une singulière rapidité. Il s'accelère communément à une certaine hauteur, et le maximum de l'accélération se rencontre dans une couche d'air dont l'elevation absolue parait varier suivant le climat. Vers l'equateur, M. de Humboldt a reconnu cette couche entre 2500 et 3500 métres d'elevation. Dans les Pyrénées, j'ai cru la trouver entre 2000 et 3000 métres. Plus haut il se ralentit de nouveau."—*Ramond, sur la Form. Barom.* p. 184.

* Professor Playfair, who had no system to support, adopts an opinion with regard to the gradation of heat quite opposite to that of Professor Leslie. It is curious that both of them draw their conclusions from the same observations of Saussure on Mont Blanc.—*Outlines of Nat. Phil.* vol. i. p. 251.

Now

Now this passage does not authorise an indefinite acceleration in the decrease of heat, as Dr. Young supposes, but only a limited acceleration to a certain height; beyond which the decrements of heat become less for a given variation of altitude. All that is said will be explained by supposing an anomaly arising from the vicinity of the earth, which ceases at such altitudes where the temperature of the air is little affected by terrestrial objects.

3. Having now premised such observations as seemed necessary for removing obstructions, and for introducing clearness into the discussion, I shall next apply to the present research the equations which have been investigated in the last Number of this Journal. The elasticity and density of the air at the earth's surface being each represented by unit, and the relative quantities of the same things at any height in the atmosphere being respectively equal to p and $\varrho = 1 - \omega$, the equations may be thus written,

$$p = \left(\frac{1 + \alpha\tau - \alpha i}{1 + \alpha\tau}\right)^3 \times \frac{1 + \alpha\tau - \alpha i + \alpha\theta}{1 + \alpha\tau},$$
$$\varrho = \left(\frac{1 + \alpha\tau - \alpha i}{1 + \alpha\tau}\right)^3. \qquad \text{(F)}$$

Here τ is the temperature at the earth's surface; i is the number of degrees of the thermometer that measures the heat absorbed when air passes from the density 1 to the density $1 - \omega$; and θ stands for all the accessions or diminutions of heat proceeding from extraneous sources, and affecting the temperature of a mass of air at the given height. These formulæ determine the equilibrium of the atmosphere, which will take place when the external pressure is equal to the elasticity. One equation more is still necessary for expressing the relation between the pressure, the density, and the elevation. Let h denote the length of the mercurial column which balances the pressure of the atmosphere, and D the density of the air at any height x; and let h' and D$'$ stand for the same things at the earth's surface: then we have

$$h = \int - \mathrm{D}\, dx.$$

Suppose that l is the length of the homogeneous atmosphere, or of a column of air having the uniform density D$'$, and equal in weight to the mercurial column h': then, $h' = l \times \mathrm{D}'$; and

$$\frac{h}{h'} = \int - \frac{\mathrm{D}}{\mathrm{D}'} \times \frac{dx}{l}.$$

In this equation l is still a variable quantity, for it depends on the temperature at the earth's surface. Now change l to denote the homogeneous atmosphere at some fixed temperature,

ture, as zero; then the length, at any given temperature τ, will be equal to $l \times (1 + \alpha\tau)$: hence

$$\frac{h}{h'} = \int - \frac{\mathrm{D}}{\mathrm{D}'} \times \frac{dx}{l(1+\alpha\tau)}.$$

We therefore obtain these formulæ,

$$s = \frac{x}{l(1+\alpha\tau)}, \qquad \text{(H)}$$

$$p = \int - \varrho\, ds = \int - (1-\omega)\, ds.$$

The equations (F) and (H) are the analytical expressions of all the physical relations known to subsist between the pressure, density, temperature, and height of a mass of air in an atmosphere *in equilibrio*. All the four things mentioned depend upon the two variable quantities i and θ; or if we put $t = i - \theta$, they depend upon i and t, which represent the heat absorbed, and the temperature lost, by the elevation of a mass of air from the earth's surface to the height it occupies. The inquiry into the constitution of the atmosphere is therefore reduced to investigating the relation that subsists between i and θ, or between i and t.

4. Dalton has supposed that a given mass of air retains the whole of its absolute heat, whatever be its position in the atmosphere, the temperature lost being just equal to the heat which enters into combination in a latent form. This hypothesis is equivalent to making $\theta = 0$, or $i = t$, in the preceding formulæ. But by an easy appeal to experience we may prove that Dalton's supposition is not agreeable to nature. In the aërial ascent of Gay-Lussac the centigrade thermometer fell from 30°·8 to −9°·5, the total depression being 40°·3, at the same time that the elasticity of the air was reduced from 1 to 0·432, and the density to $\frac{1}{2}$ very exactly. Now from the formula

$$\varrho = \left(\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}\right)^3$$

we get

$$i = \frac{1+\alpha\tau}{\alpha} \cdot (1 - \varrho^{\frac{1}{3}}):$$

and, when $\varrho = \frac{1}{2}$, and $\tau = 31°$, it will be found that $i = 61°$, or about 21° greater than the observed temperature. It appears therefore that, in the atmosphere, the temperature that prevails is much less than the heat absorbed by the rarefaction of the air.

In a memoir on the Theory of Sound, published in 1807*, M. Poisson has supposed that, when air suffers a small condensation or rarefaction ω, the heat evolved or absorbed is

* 14e Cahier *de l'Ecole Polytechnique.*

proportional

proportional to ω and equal to $A \times \omega$; and upon this supposition he has found that the coefficient A must be equal to 116° in order to make the computed velocity of sound agree with the observed quantity. Now if we make $\omega = \frac{1}{2}$, in the formula $116° \times \omega$, the result is 58°, not much different from 61° the quantity already found.

Conceive an atmosphere such as Dalton supposed, in which a given mass of air, whatever be its height, retains the whole of its absolute heat. As every parcel of air has the source of its temperature entirely within itself, neither communicating heat nor receiving any, it follows that its elasticity will be the same whether it be in motion or at rest. Wherefore, the atmosphere being *in equilibrio*, if a velocity in any direction be communicated to a mass of air in it, the elasticity would at every instant balance the external pressure, and there would be no force tending to alter in any respect the velocity impressed. Motion once begun, would, in such an atmosphere, be perpetual.

What has just been said applies equally to an atmosphere in every part of which the same constant temperature prevails. For in this hypothesis also the elasticity and the pressure of a mass of air would constantly balance one another, whether in a state of motion or of rest. In the case of intestine motion accidentally excited, there is no means provided, in either of the atmospheres we have mentioned, of bringing back the air to a state of rest.

It may be observed that the two atmospheres we have been considering are not mere theoretical fictions, with regard to which it may be doubted whether they can possibly exist or not. Both of them are physically possible: and they may both be constructed by means of operations that can be performed with air. If we have a close vessel containing air of which the density is unit, and enlarge the dimensions of the vessel till the density is reduced to $1-\omega$, at the instant of the rarefaction, and before any heat is received through the medium of the containing vessel, the elasticity of the confined air will be equal to $(1-\omega)^{\frac{4}{3}}$, and the function for temperature to $(1-\omega)^{\frac{1}{3}}$. In this state the dilated air retains the whole of its absolute heat, the temperature lost being identical with the heat absorbed. Now if we put $(1-\omega)^{\frac{4}{3}}$ for p in the equations (H), we shall determine the height x, or the place in the atmosphere of air having the density $1-\omega$. By repeating like operations for every degree of dilatation, it is evident that the entire atmosphere may be constructed by the guidance of

nature alone, without any hypothetical assumption that may possibly involve a physical absurdity. This is one of the atmospheres considered in the paper already cited: it corresponds to the supposition of $m = 3$*; the total height is about 20 miles; and the horizontal refraction will be found equal to $33'\ 7''$ at the mean temperature of 50° of Fahrenheit, and the barometric pressure of 30 inches.

Again, if we allow the temperature lost by the rarefaction in the close vessel to be completely restored, the relative elasticity within the vessel will be equal to the relative density, and the function for temperature will be equal to unit. In this condition of the rarefied air, the temperature is always the same whatever be the degree of dilatation. If in the equations (H) we put $p = \varrho = 1 - \omega$, that is, if we suppose that the relative pressure is equal to the relative density, we shall obtain

$$p = c^{-s},$$
$$\varrho = c^{-s};$$

and these formulæ determine the height of air having a given degree of rarefaction in an atmosphere in every part of which the same temperature prevails. This atmosphere, as it is the simplest that can be imagined, so it is the first that presented itself to the consideration of geometers: its height is unlimited; and the horizontal refraction is equal to $37'\ 34''$.

It is however certain that neither of the two atmospheres we have been considering coincides with that of nature. This point seems to be sufficiently established by having proved that in both cases the equilibrium is unstable, and would be overturned by the least motion communicated by any external cause. But other considerations confirm the same conclusion. For, in one, the temperature is the same at all heights, whereas in the real atmosphere the temperature diminishes as the height increases. And, in the other, the temperature in ascending is precisely equal to the heat absorbed by the rarefaction; but, in nature, the first of these quantities is much less than the second. But although neither of the two atmospheres agrees exactly with that of nature, they are not undeserving of notice, both on account of the properties they possess in common, and because, as will soon appear, they are the limits on either hand between which the real atmosphere is contained.

Returning to the rarefied air in the close vessel, the elasticity is equal to $(1 - \omega)^{\frac{4}{3}}$ at the instant of rarefaction, and to $1 - \omega$ when the air has acquired the same temperature that

* Phil. Trans. 1823, p. 439.

prevails

prevails on the outside. In passing from one of these limits to the other, the elasticity will successively acquire every intermediate degree of magnitude. If therefore $\varphi(\varrho)$ or $\Phi(1-\omega)$ denote a function of the density, the value of which is between the limits $(1-\omega)^{\frac{4}{3}}$ and $(1-\omega)$, the elasticity of the air within the vessel will at some instant between the two extreme states be equal to $\varphi(\varrho)$ or $\varphi(1-\omega)$; and by means of the equations (H), we may as before construct an atmosphere in which the elasticity and external pressure will be each equal to $\varphi(1-\omega)$ when the density is equal to $1-\omega$. Thus innumerable atmospheres may be imagined, which are all physically possible. Their existence demands nothing except operations that may be performed with air. In all of them the pressure and temperature are not hypothetically determined by assuming algebraic formulæ, which may or may not be consistent with the properties of air; they are deduced from real considerations, namely, the changes of volume, and the transference which the equilibrium of heat requires. The air in the close vessel at the instant of the rarefaction retains the whole of its absolute heat without increase or diminution; but at any succeeding point of time, there is an increase of heat received through the medium of the containing vessel, which affects the temperature only. It follows, therefore, that in all the intermediate atmospheres a mass of air receives an increase of its absolute heat in ascending above the earth's surface, and the loss of temperature at any height is less than the heat absorbed by the rarefaction. As experience shows that this property is one of the characters of the real atmosphere, the cases to which it belongs deserve to be particularly considered.

Suppose that, in an atmosphere such as we have mentioned, a given mass of air moves upwards. If it is warmer than the particles in contact with it, its temperature will be continually lessened both by transference and absorption, and will finally be reduced to an equality with that of the contiguous fluid. If the air has not lost all its motion when arrived at this point, we may compare it in its further progress with the portions of air occupying the same successive places in a state of rest and equilibrium. It is evident that the external pressure will be the same at the same point of space, whether the air be in motion or at rest. But the extraneous heat communicated at any point to the air in motion will be less than what would be received by a mass occupying the same place permanently in a state of rest. For the particles in their momentary passage will carry off less heat than they would have acquired by remaining stationary and exposed to the full effect of the heating causes. Hence every mass of air at rest and *in equi-*

librio will have more acquired heat than the ascending mass when it comes to occupy the same place; and the difference will be more considerable when the velocity is greater. Now the pressures being the same in both cases, it follows that, in the former case when more heat is acquired, the elasticity, which in a state of rest balances the external pressure, will be greater than the elasticity in the latter case. Wherefore the elasticity of the ascending mass of air is less than the external pressure; and the difference of these two forces opposing the ascent, the velocity will be continually lessened and finally destroyed. The reverse of all this will take place when a mass of air moves downwards. In descending it will lose heat; its elasticity will decrease faster than the pressure augments; and the velocity will be extinguished when these two forces are reduced to an equality. It thus appears that an atmosphere constituted as we have supposed, contains a principle of stability, by means of which vertical motions upwards or downwards excited accidentally in the air will be destroyed.

In reasoning on this subject, if we confine our attention to the powers really existing in nature, that is, to the actual heat of the atmosphere and the different ways of distributing it by the variation of pressure and density, the atmospheres we have already considered are all that can possibly be admitted. A full enumeration of all the cases that can be imagined would, indeed, lead us to another class of atmospheres, in which the temperature lost in ascending is greater than the heat absorbed by the rarefaction. But it seems impossible to conceive in what manner the loss of temperature can be carried beyond the heat of combination merely by transference between the contiguous air, and without assuming hypothetically some extraneous source of cold. It is the less necessary to dwell on this point because the real atmosphere cannot be included in this class, which possesses properties quite opposite to the class already mentioned. In an atmosphere of this kind, a mass of air in ascending would lose heat, and the elasticity would decrease faster than the pressure; in descending, it would acquire heat, and the elasticity would increase faster than the pressure; in both cases there would be an augmentation of velocity. Motion upwards or downwards once begun, would be accelerated, instead of being retarded and destroyed.

It has been shown that, in fact, the temperature lost in ascending in the atmosphere is less than the heat absorbed by the rarefactiou of the air. We have proved that this is the only constitution of an atmosphere which is stable, that is, which contains a principle capable of destroying motions upwards

wards or downwards when they have been accidentally excited. The extreme limits between which every atmosphere possessing these properties is contained are, on the one hand, the atmosphere imagined by Dalton, in which every mass of air retains the whole of its absolute heat without increase or diminution; and, on the other, the atmosphere of equable temperature, in which a mass of air has the whole heat absorbed by rarefaction restored to it. In these extreme atmospheres the only action which the aggregate exerts upon its parts is pressure; and every partial mass, having the sources of temperature entirely within itself, is indifferent to motion or rest, since in either case the elasticity will equally balance the external pressure. In any of the intermediate atmospheres, when a mass of air shifts its place, its elasticity varies both on account of the change of volume and the transference of heat; and as these two causes produce effects contrary to one another, there is introduced a principle of stability tending to destroy vertical motions upwards or downwards. By the foregoing reasoning the investigation of the true atmosphere is at least brought within certain limits. In proceeding further, we must have recourse to other phænomena, in order to particularize the individual case that alone agrees with nature in all its properties. But we may here observe that the principle of stability, which is evanescent in both the extreme cases, must first increase and then decrease in passing from one to the other. At some intermediate point it will therefore operate with a maximum effect; and it is not unreasonable to conjecture that this particular case, merely because it is single, will be found to agree with the atmosphere of nature. This, however, is a point to be inquired into, not one conclusively established.

Aug. 2, 1825. [To be continued.] JAMES IVORY.

XII. *Abstracts of a Series of Papers lately read before the Royal Society, &c. on the Magnetism developed by Rotation. By Messrs.* BARLOW, CHRISTIE, BABBAGE, HERSCHEL, *and* MARSH*.

On the Magnetism imparted to Iron Bodies by Rotation. By PETER BARLOW, *Esq. F.R.S.*

THE author's attention having been recalled to the consideration of the effects of rotation in altering the magnetic influence of iron, in the course of speculations on the cause of the

* The great importance of the experiments and investigations detailed in

the rotation of the earth's magnetic poles, and knowing at the same time that Mr. Christie had found a permanent change in the magnetic state of an iron plate by a mere change of position on its axis, it seemed to him highly probable that this change, due only to a simple inversion, would be increased by rapid rotation. On trial, however, it was found that the effect produced was merely temporary. The experiments at first were made with a thirteen-inch mortar-shell fixed to the mandrel of a powerful turning lathe, worked by a steam-engine in the royal arsenal at Woolwich.

This being made to revolve at the rate of 640 turns per minute, the needle was deflected out several degrees, and there remained stationary during the motion of the ball, but returned immediately to its original position on ceasing the rotation. On inverting the motion of the shell, an equal and contrary deflection took place.

As the law of the phænomena was not evident with this disposition of the apparatus, and the shell was found too heavy for perfect safety, a Shrapnell shell of eight inch diameter was mounted in a proper apparatus (described in the paper), and a number of experiments made, the law of which, however, still seemed anomalous, till the idea occurred of neutralizing the earth's action on the needle; when the anomalies disappeared, and the general law of the effect was placed in evidence. The needle being made a tangent to the ball, if the ball was made to revolve *towards* the needle (whatever was the direction of the axis of rotation), the north end of the latter was attracted, and if the contrary way, repelled. In the two extremities of the axis there was found no effect; while in two opposite points at right angles to the axis, the effect was a maximum, and the direction of the needle was to the centre of the ball.

The author then proceeded to show how the results, which before appeared anomalous, agree with this general view, and closed his communication with some theoretical views of their general bearing on the subjects of the earth's magnetism, which he thought there were strong reasons for believing to be of the *induced* kind; and although it appeared to him doubtful whether the anomalies observed in the variation of the needle on the earth's surface can ultimately be referred to this cause, yet he observed that one condition essential to the production

in these papers has determined us to give in a connected form the official abstracts of them, as read before the Royal Society, from the Quarterly Journal of Science. We have added, to complete the series, Mr. Marsh's account of Mr. Barlow's repetition of M. Arago's late experiments, from the Edinburgh Philosophical Journal.

of

of these phænomena holds good in the case of the earth, viz. the non-coincidence of its polarized axis with that of its diurnal rotation.

On the Alteration in the Magnetism of an Iron Plate, occasioned by a Rotation on its Axis. By S. H. Christie, *Esq.*

The effects observed and described in this paper, although minute in themselves, appeared, in the author's opinion, to point out a species of magnetic action not hitherto described. It has long been well known that striking, twisting, or filing iron, in different directions, with regard to the magnetic axis, materially influences its polarity, but it does not appear to have been remarked that the simple rotation of iron in different directions has any such influence. This, however, the author has ascertained to be the case, and that the laws which govern this peculiar action are so regular, that there can remain no doubt of a corresponding regularity in their causes.

The attention of the author was first drawn to these phænomena by some apparent anomalies in the magnetic action of an iron plate on the compass, observed in the course of a different investigation. In order to avoid or allow for the disturbing influence of partial magnetism in the iron, it became necessary to attend minutely to the position of certain points in its circumference, which corresponded to the maxima and minima of this magnetism. It was then found that these points were not constant, but shifted their position as the plate was made to revolve in its own plane; or, in other words, that a plate which, in a given position, produced a certain deviation in a compass, no longer produced the same deviation after making an exact revolution in its own plane, although brought to rest, and every part of the apparatus restored precisely to its former place.

It appeared from this, that the revolution of the plate in its own plane had an influence on its power of deviating the needle independent of the partial magnetism of particular points in it; and the justice of this idea was proved by giving it a rotation in an opposite direction, when the effect on its directive power was also reversed.

The change produced by rotation in the directive power of the plate was found to be a maximum when its plane was parallel to the line of dip on the magnetic axis, and at the same time as little inclined to the horizon as this condition would allow; but when the plane of the plate was parallel to the horizon, the effect was diminished in the ratio of 5 to 1, and when perpendicular to the horizon, and coincident with the magnetic meridian, was altogether destroyed.

The

The author, having satisfied himself of the reality and constancy of this effect, in different plates, and of the necessity of referring it to a peculiar agency of the earth's magnetic power on the molecules of the plate, proceeded to ascertain the laws, and measure the quantities of the *deviation* due to rotation (so he terms it) in various positions; and detailed a great number of experiments, with their numerical results, arranged in the form of tables.

From these he deduced the following general law; viz. that the deviation due to rotation in *a dipping needle* "will always be such, that the sides of the equator of such dipping needle will deviate in a direction contrary to the directions in which the edge of the plate moves, that edge of the plate nearest to either edge of the equator producing the greatest effect."

The results of this law, it may be here observed, are in many cases coincident with those of the following: conceive the dipping needle orthographically projected on the plate. Then will the *deviation due to rotation* of the projected needle take *place in a direction opposite to that of the rotation itself.*

The author then proceeded to a theoretical investigation of the effect of a plate of soft iron, having within it two poles developed in given positions, and acting (in addition to the usual magnetic action of soft iron) on a needle of infinitely small dimensions, in the plane of the plate. He referred the whole ordinary action of the iron to its centre, and supposed that this is *attractive* on both poles of the needle; but the extraordinary action on that of the newly-developed poles he supposed to reside in them, and to be attractive or repulsive, according as they act on the poles of the needle of the same or opposite names with themselves. On this hypothesis, assuming symbols for the co-ordinates of the plate's centre, the distance separating the newly-developed poles in the plate, and the angle which the line joining them makes with the direction of the needle, &c., he deduced (from the known laws of magnetism) formulæ, expressing the horizontal deviations of the needle:—first, on the supposition of a rotation in one direction; secondly, on that of a rotation in the opposite; and thirdly, in that of no rotation at all. From these, by comparing them with a few of the observations, he deduced numerical values for the constants of the formulæ, and then employed them to compute the deviations due to rotation in all the rest. He regarded the discrepancy between the calculated and observed results, as in few cases, larger than what he considered may be fairly attributed to error of observation; and that the theory above stated is at least a general representation of what passes in fact: admitting, however, that it does not give the exact position

tion of the point where the deviation due to rotation vanishes, and suggesting partial magnetism in the iron plate used as one mode of accounting for the difference. At all events, by an examination of the case on the ordinary supposition of induced magnetism in the iron, he showed that a greater coincidence between theory and fact would not result from that hypothesis than from the one here employed.

He then proceeded to inquire into the degree of permanence of the polarity thus produced in iron by rotation; from which inquiry it appeared that (at least during 12 hours after the plate was brought to rest) the influence of a single rotation had scarcely suffered any diminution. It appeared also that the effect is so far from depending on the rapidity of the motion, that the plate can scarcely be made to revolve so slowly as that the whole effect shall not be produced.

Lastly, by a slight change in the formulæ, the results of computation, it is found, can be made to agree with observation to a degree of exactness as near as can be wished. This change consists in the omission of certain terms introduced by the theory, and the author regards it as very possible so to modify the theory as to get rid of them.

The author closed this communication with an appendix comparing the magnetic effects produced by slow and rapid rotation. The result of the comparison was, that the forces exerted on the needle during rapid rotation were always in the same direction as those derived from the slowest rotation, and which continue to act after the rotation has ceased, but were greater in intensity, and that the former effects were such as might have been looked for from a knowledge of the latter.

An Account of the Repetition of M. Arago's *Experiments on the Magnetism developed during the Act of Rotation. By* Charles Babbage, *Esq. F.R S., and* J. F. Herschel, *Esq. Sec. R.S.*

The experiments of M. Arago having excited much interest, the authors of this communication were induced to erect an apparatus for their verification; and after a few trials, they succeeded in causing a compass to deviate from the magnetic meridian, by setting in rotation under it plates of copper, zinc, lead, &c.

To obtain more visible and regular effects, however, they found it necessary to reverse the experiment, by setting in rotation a powerful horse-shoe magnet, and suspending over it the various metals, and other substances to be examined, which were found to follow with various degrees of readiness the motion of the magnet. The substances in which they succeeded

in developing signs of magnetism were, copper, zinc, silver, tin, lead, antimony, mercury, gold, bismuth, and carbon in that peculiar metalloidal state in which it is precipitated from carburetted hydrogen in gas works. In the case of mercury, the rigorous absence of iron was secured. In other bodies, such as sulphuric acid, rosin, glass, and other non-conductors, or imperfect conductors of electricity, no positive evidence of magnetism was obtained.

The comparative intensities of action of these bodies were next numerically determined by two different methods, viz., by observing the deviation of the compass over revolving plates of great size cast to one pattern, and by the times of rotation of a neutralized system of magnets suspended over them; and it is curious that the two methods, though they assigned the same order to the remaining bodies, uniformly gave opposite results in the cases of zinc and copper, placing them constantly above or below each other according to the mode of observation employed.

Our authors next investigated the effect of solution of continuity on the various metals: in the course of which M. Arago's results of the diminution of effect by division of the metallic plates used were verified; and the further curious fact ascertained, that re-establishing the metallic contact with other metals restores the force, either wholly or in great measure; and that even when the metal used for soldering has, in itself, but a very feeble magnetic power, thus affording a power of magnifying weak degrees of magnetism. The law of diminution of the force by increase of distance was next investigated. It appears to follow no constant progression according to a fixed power of the distance, but to vary between the square and the cube.

The remainder of this paper was devoted to some able and elaborate reasoning on the facts detailed.—The authors conceive that they may be all explained without any new hypothesis in magnetism, by supposing simply that time is requisite both for the development and loss of magnetism; and that different metals differ in respect, not only of the time they require, but in the intensity of the force ultimately producible in them; and they apply this explanation not only to their own results, but to those obtained by Mr. Barlow in his paper on the rotation of iron.

Experiments on the Magnetism produced by Rotation. By S. H. Christie, *Esq., in a Letter to Mr.* Herschel.

Mr. Christie, in this communication, gave an account of some experiments on the development of magnetism in copper by

by rotation. He corroborated by his own experience the results obtained by Mr. Herschel, in which a disc of copper was set in rotation by the rotation of one or more magnets beneath it, both in the case where poles of the same name were immediately below the disc, and when of a contrary name. The actions appeared equally intense in both cases; and from this circumstance, he concludes the magnetism thus communicated to the copper to be extremely transient. The experiment was varied by combining the revolving magnets differently, and the results were stated.

The next experiments of Mr. Christie were directed to the determination of the law according to which the force diminishes as the distance between the disc and magnets increases. It seems to follow from these experiments, that when a thick copper plate is made to revolve under a small magnet, the force tending to deviate the needle is directly as the velocity, and inversely as the fourth power of the distance; but that when magnets of considerable size are made to revolve under thin copper discs, the diminution follows more nearly the ratio of the inverse square of the distance, or between the square and the cube, though not in any constant ratio of an exact power.

The author then investigated the law of force when copper discs of different weights are set in rotation, which, for small distances, appear proportioned to the weights of the discs, but for smaller ones appear to vary in some higher ratio.

Account of the Repetition of M. Arago's *Experiments on the Magnetism developed during the Act of Rotation. By Messrs.* Barlow *and* Marsh.

The experiments by Mr. Barlow on the magnetism imparted by rotation, described in p. 93, were begun in Dec. 1824; and it was not until April 1825 that he was informed of M. Arago's rotative experiments on copper and other metals. "The latter were not known in England," Mr. Marsh states, "until M. Gay-Lussac's visit to London at the time above stated."—"I am not aware," he continues, "of the precise nature of these experiments; and shall, therefore, only endeavour to describe those which I have assisted Mr. Barlow in making, and which he founded on the description he had received: they may, therefore, be considered as the experiments of M. Arago repeated, and varied as different circumstances occurred to suggest new ideas. The account he had of M. Arago's experiment, was that, by placing a copper plate upon a vertical spindle, the plate being horizontal, and then placing just above it a light compass needle, but independent, of course, of the

plate; on causing the spindle and plate to revolve, the needle was considerably deflected, and more and more as the velocity was increased; so that, when the plate was put into rapid rotation, the needle also began, after a few vibrations, to revolve, and at length with considerable velocity.

"1. In order to repeat this experiment, I connected the wheel of my turning lathe with a vertical spindle, which I could make revolve forty-five times per second; and on this I placed a thin copper plate, about six inches in diameter, and over this a needle about five inches long, shut up in a close box, about one inch, or rather less, above the plate. When putting the lathe in motion, I found it to deflect the needle about five points, the deflection being always in the same direction as the motion of the plate, but we could not cause it to revolve. The needle was, therefore, partly neutralized by a bar magnet, and the experiment repeated. We then very soon obtained a considerable rotatory motion in the needle; and, by using a larger and heavier plate, the same was produced afterwards without neutralizing the needle.

"2. Another experiment, which was mentioned as one of M. Arago's, and which I repeated, was, by interposing a plate of iron between the copper plate and the needle. In this case, no effect could be produced on the needle by the rotation of the copper plate, the iron clearly intercepting the action.

"3. The only other experiment that I am aware of as originating with M. Arago, at least that I repeated, was the rotation of a plate cut into radii like a star, which was said, as I understood, to produce no effect: this, however, was not the case in my experiments,—it certainly produced a less effect, but, I think, not less than might have been anticipated, from the quantity of copper thus taken away.

"4. I now tried a zinc plate instead of a copper plate, and the effect was nearly the same as before, but a little less.

"5. An iron plate was now substituted, and the effect was considerably greater than with the copper plate.

"6. The copper plate was again replaced, and a brass needle placed in the box. Some motion was obtained, but it was very equivocal, so that I cannot venture to say that it was certainly due to the rotation.

"7. A heavy horse-shoe magnet was now suspended by a line from the ceiling; and it was put in rotation by the revolution of the copper plate, a paper screen having been first interposed between them.

"8. One copper plate was suspended over another, but no motion was obtained; and the same took place when the copper plate was suspended over an iron one.

"9. A bar

"9. A bar magnet, rather shorter than the diameter of the copper plate, was fixed horizontally to the upright spindle; and being made to revolve, the plate very soon acquired rotation. A paper screen was, in this, as in the preceding experiments, interposed between the plate and magnet.

"10. The plate was now applied immediately to the axis of the lathe, so as to cause it to revolve vertically, and the needle placed near to it; but no motion took place, till, by nearly neutralizing the needle, and bringing either of its poles directly to the plate, it then always deviated in the direction of the motion of the plate; whichever pole of the needle was directed to the former. The needle, of course, therefore, deviated different ways (all other things being the same), when it was above or below the axis; but in the direct horizontal line of the axis no motion in the needle took place.

"11. The above are the principal experiments that I assisted in making by revolving the plate; but these having suggested to Mr. Barlow that all the results obtained might be explained, by supposing that there existed a slight magnetic power in copper, and in the various metals which had a tendency to draw the needle after the plate, or the latter after the former, he endeavoured to exhibit this by direct experiment, independent of revolution. With this view, he neutralized a needle very accurately; and then applying very near to its poles the end of a round brass ruler, the attraction of the latter was obvious,—it drew the needle several degrees,—then, withdrawing it, and catching the needle again in its returning vibration, it was drawn out some further degrees; and, in a very short time, the deflection was converted into a revolution, which, by alternately presenting and withdrawing the needle, was at length rendered very rapid.

"12. The same result was obtained by two or three different pieces of brass; but there were other pieces, although of the same size and form, which had little or no effect.

"The following experiment is due to Mr. Sturgeon, of Woolwich.

"13. A thin copper plate or wheel, about five or six inches in diameter, was suspended very delicately on an axis, and then one side a little weighted, in order to give it a tendency to oscillate. The heavy point was now raised level with the axis, and the number of vibrations the plate made before it came to rest were counted. The same was again done, with this difference only, that the vibrations now took place between the poles of a horse-shoe magnet; and the number of them before the plate came to rest, was very little more than one half of what they were in the former instance.

"This

"This is the converse of M. Arago's experiments, in which he shows the effect of copper and other metallic rings, in diminishing the number of oscillations of a magnetic needle.

"14. If, instead of a horse-shoe magnet, the contrary poles of two bar magnets be used, the effect is the same as before; but, if the poles of the same name, viz. both north or both south, be employed, then the effect is scarcely perceptible. This is an important result, as it shows that the effect is not due to any kind of resisting medium, as was supposed in the first instance."—*Edin. Phil. Journ.*

XIII. *On the Conditions of Possibility, Arbitrary Functions, and Complete Solutions of Periodical Functional Equations.* By John Herapath, *Esq.*

SOLUTIONS of functional equations have been considered to be of three kinds; *particular*, *general*, and *complete*. It has also been usual to consider the complete solution of any equation of the first order, as for instance of

$$\mathrm{F}\{x, \psi x, \psi\alpha x, \psi\alpha^2 x, \ldots \psi\alpha^{n-1}x\} = 0 \qquad (1)$$

to contain $n-1$ arbitrary functions; and with respect to the form of F, it seems to have been tacitly admitted to be quite unlimited. Having been led by my inquiries to different conclusions, I purpose in the present paper to examine the conditions of possibility of (1), the limitations to the form of F, and to give a simple direct method of obtaining the complete solution. I regret only that my very confined limits will oblige me to be less explicit in my exemplifications than I could wish.

Of the Conditions of Possibility.

When any equation of the form of (1) is given, it may be put under the form of

$$\psi x = f\{x, \psi\alpha x, \psi\alpha^2 x, \ldots \psi\alpha^{n-1}x\} \qquad (2)$$

and there must simultaneously subsist the $n-1$ following equations,

$$\begin{aligned}
\psi\alpha x &= f\{\alpha x, \psi\alpha^2 x, \psi\alpha^3 x, \ldots \psi x\}\\
\psi\alpha^2 x &= f\{\alpha^2 x, \psi\alpha^3 x, \psi\alpha^4 x, \ldots \psi\alpha x\}\\
&\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots \qquad (3)\\
&\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\quad\cdots\\
\psi\alpha^{n-1}x &= f\{\alpha^{n-1}x, \psi x, \psi\alpha x, \ldots \psi\alpha^{n-2}x\}
\end{aligned}$$

If now these $n-1$ right-hand functions be substituted for their values $\psi\alpha x, \psi\alpha^2 x, \ldots$ in (2), the resulting right-hand member

member of the equation ought of itself to eliminate all the quantities x, $\psi\alpha x$, $\psi\alpha^2 x$, . . . except ψx; and should become simply ψx. If this does not take place, the equation is impossible. For that the right-hand result in a possible equation should reduce itself to ψx is evident; because to ψx alone it is equal, and the functions of x with which $\psi\alpha x$, $\psi\alpha^2 x$, . . . are in this final result incorporated, contain every possible change, by the substitution of αx, $\alpha^2 x$, . . . for x, which they can have, and are combined in every possible way the properties of f admit: they want therefore no one element or point in variety or form of combination which the elimination of x, $\psi\alpha x$, $\psi\alpha^2 x$, . . . that must unavoidably take place requires; and hence of course it ensues.

Thus in $\psi x = fx.\psi\alpha x$ where $\alpha^2 x = x$ the condition of possibility is $fx.f\alpha x = 1$; and if this does not hold, the equation is impossible. For instance, $fx = 2$ giving $\psi x = 2\psi\alpha x$ is an impossible condition. So in the same equation, where $\alpha^n x = x$, the condition of possibility is $fx.f\alpha x \ldots . f\alpha^{n-1}x = 1$; without which it is impossible. And thus we may examine the conditions of possibility in more complex cases.

Of the Limitations to the Form of f in (2).

In any equation of the form of $\psi x = f\psi\alpha^r x$ where $\alpha^n x = x$ it is easily shown that $f^{\frac{n}{r}} x = x$, or that f must be a periodic of the $\frac{n}{r}$th order. If therefore in (2) we regard the right-hand member as a function of any term only, $\psi\alpha^r x$ for example, the other terms and the functions of x with which this term is combined, must be so involved as with their respective properties to make it a function of the $\frac{n}{r}$th order.

This is evident in the equation

$$\psi x = \frac{fx^2}{fx - \psi\alpha x}$$

where $\alpha^3 x = x$ and fx is any symmetrical function of x, αx, $\alpha^2 x$; and likewise in the equation

$$\psi x = fx.\psi\alpha x + f_1 x$$

where $\alpha^2 x = x$, and the conditions of possibility are $fx.f\alpha x = 1$, and $fx.f_1\alpha x = -f_1 x$.

On the Number of Arbitrary Functions in the complete Solution of (2).

For x in (2) put $\psi^{-1}x$, and the equation becomes

$$x = f\left\{\psi^{-1}x, \psi\alpha\psi^{-1}x, \ldots \psi\alpha^{n-1}\psi^{-1}x\right\}$$

But

But $\psi\alpha\psi^{-1}x$, $\psi\alpha^2\psi^{-1}x$, . . . are manifestly the first, second, . . . orders of the functional root of a certain periodic of the nth order. Let this periodic be $\beta^n x = x$, and the equation will become

$$x = f\{\psi^{-1}x, \beta x, \beta^2 x, \ldots . \beta^{n-1}x\} \qquad (4)$$

Now because

$$\beta x = \psi\alpha\psi^{-1}x, \beta^2 x = \psi\alpha^2\psi^{-1}x, \beta^3 x = \psi\alpha^3\psi^{-1}x, \ldots . (5)$$

it is evident that the arbitrary function or functions, which might exist in the determination of ψ generally, from any one alone of these equations, must disappear when ψ is considered to be determined, as it should be, from all the equations (5) simultaneously. Consequently if β has no arbitrary function, ψ has not; and *vice versâ*, if ψ has no arbitrary function, neither has β. Again, because of the invariable relation which the above equations (5) establish between ψ and β, considering the form of α invariable, β determines the form of ψ, and ψ the form of β. Let us now suppose α, instead of having a constant form, has, and likewise ψ, every possible form. This will give to β its utmost generality, and at the same time render it independent of ψ. With such views (4) becomes

$$x = f_1\{\beta x, \beta^2 x, \beta^3 x, \ldots \beta^{n-1}x\} \qquad (6)$$

Moreover, because β cannot be otherwise than a periodic of the nth order, its generality, if there be any difference, should be greater when its form is determined from the simple condition $\beta^n x = x$, than when from this condition coupled with the properties of f_1. But when β is determined from $\beta^n x = x$ I have demonstrated, (Annals for December 1824, p. 424,) that its complete form has but one arbitrary function. Therefore ψ in (2), whose form is determined by that of β, has only one arbitrary function. And this must be equally true for each and all the $n-1$ contemporaneous equations (3), which together with (2) involve x under every change from $\alpha^o x$ to $\alpha^{n-1}x$. Hence *the complete solution of any periodical functional equation of the first order, as* (2), *and likewise the complete solution of any periodical equation of any order, as* (6), *has but one arbitrary function, containing under it every possible transformation x can have in the n simultaneous equations.*

It will perhaps have been observed (Philosophical Magazine for September 1824, p. 199) that I once held a different opinion respecting the number of arbitrary functions in the complete solution; but, as I then hinted, my ideas were not sufficiently matured to speak confidently; and I was led away by analogical

analogical views, which have been the means of misleading probably every other individual who has attempted the discovery.

This determination of the number of arbitrary functions, which has been so great a desideratum, might be differently and more concisely demonstrated, but I have preferred a more explicit method, lest, by not comprehending every prominent point in detail, doubt should rest on the result. I shall presently give some examples of its accuracy.

On the direct complete Solution of (2) *and* (6).

In the two following methods, by which the complete solution may be obtained, each having under peculiar circumstances its advantages, I shall state them separately.

First method.—For $\psi\alpha x$ in (2) put $\psi\alpha x \cdot \varphi x^v$, which is equal to $\psi\alpha x$ when $v = 0$. Substitute αx, $\alpha^2 x$, ... $\alpha^{n-1} x$ successively for x, and from the n equations eliminate $\psi\alpha x$, $\psi\alpha^2 x$, ... $\psi\alpha^{n-1} x$. The result will be an equation of the form

$$f_1\left\{x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x, \psi x, \varphi x^v, \varphi\alpha x^v, \ldots \varphi\alpha^{n-1} x^v\right\} = 0$$

Differentiate this with respect to v, divide the result by dv, and put $v = 0$, which gives an equation of the form

$$f_2\left\{x, \alpha x, \ldots \alpha^{n-1} x, \psi x, \varphi x, \varphi\alpha x, \ldots \varphi\alpha^{n-1} x\right\} = 0 \qquad (7)$$

comprehending the log. in φ. From this equation the form of ψx may be determined.

Second method.—For $\psi\alpha x$ in (2) put $b\psi\alpha x + v\varphi x$, which coincides with $\psi\alpha x$ when $v = 0$ and $b = 1$. Pursue then the same course of substitution and elimination as in the first method; and after differentiating with respect to b and v, dividing by db, and including $\frac{dv}{db}$ in the arbitrary function, we shall have an equation similar to (7), from which ψx is to be determined.

In (6) we have merely to put

$$\psi\alpha\psi^{-1}x, \psi\alpha^2\psi^{-1}x, \ldots \psi\alpha^{n-1}\psi^{-1}x$$

for

$$\beta x, \quad \beta^2 x, \quad \ldots \beta^{n-1} x$$

and then putting ψx for x, the equation comes under the form of (2), from which ψx may be determined, and of consequence βx; because αx is any periodic function of the nth order taken at pleasure, provided no order of it αx, $\alpha^2 x$, ... become $= x$, unless it be the nth.

These solutions are complete; for they contain an arbitrary

function of every possible change x can undergo, combined in every possible way the n contemporaneous equations require.

It should be observed, that one of the two above methods may under certain very simple circumstances give a resulting equation of identity, in which ψx eliminates itself; but I believe this can never happen with both methods at the same time. We may likewise add, that though no constants or symmetrical functions of x, αx, $\alpha^2 x$, . . . $\alpha^{n-1} x$ appear in the proposed equation, they may nevertheless exist in it in any manner we please, consistent with the conditions of possibility.

I shall now exemplify the above results with two or three cases, which will give me an opportunity of correcting an inference in one of my former papers, and of introducing a method or two of obtaining the complete from a particular solution. We will commence with the latter method.

Suppose fx is a particular solution of any functional equation whatever. Then it is plain there can be no expression which may not be equated with $fx.\varphi x$ or $fx + \varphi x$, whichever we please. Consequently, if this product or sum be put for ψx in the given equation, the properties of this equation will restrict the unlimited generality of φ just as much as the complete solution requires. For example, in the equation

$$\psi x + fx.\psi \alpha x = 0 \qquad (8)$$

in which $\alpha^2 x = x$ and the condition of possibility is $fx.f\alpha x = 1$, a particular example is $1 - fx$. Multiplying this by φx and substituting $(1-fx)\varphi x$ for ψx and $(1-f\alpha x)\varphi \alpha x$ for $\psi \alpha x$, we shall at length find $\varphi x = \varphi \alpha x$.

Therefore the property of φ must be such that, whether it be a function or any system of functions and operations of any kind on x, it must give the same result when performed on x or on αx. In matters of such unbounded generality it would, perhaps, be too much to say, that ϕ confined to what is usually understood by functional operations could answer every possible case. However, as we are speaking of functions, we will suppose the conditions of ϕ may be effectually fulfilled by some function. It will then of course be any symmetrical function of x and αx; and the complete solution will be

$$\psi x = (1-fx).\{\phi x + \phi \alpha x\}$$

which coincides with

$$\psi x = \varphi x - fx.\varphi \alpha x$$

obtained by the second of our general methods. For both solutions have the same properties; and if respectively divided by $1 - fx$, are arbitrary symmetrical functions of x, αx.

As

As a second example, let us take

$$\psi x + fx \cdot \psi \alpha x = f_1 x$$

αx being still a periodic of the second order, and the conditions of possibility $fx \cdot f\alpha x = 1$, and $fx \cdot f_1 \alpha x = f_1 x$. The simplest solution of this equation is $\frac{1}{2} f_1 x$. If therefore we seek the complete solution by assuming $\psi x = \frac{f_1 x}{2} + \varphi x$, we shall ultimately have

$$\varphi x + fx \cdot \varphi \alpha x = 0$$

an equation of precisely the same form as (8). Hence the complete solution of our last equation is

$$\psi x = \tfrac{1}{2} f_1 x + (1 - fx) \cdot \{\varphi x + \varphi \alpha x\} \text{ or } = \tfrac{1}{2} f_1 x + \varphi x - fx \cdot \varphi \alpha x$$

the latter form being that which I have deduced by our second method (Phil. Mag. for September 1824), and the former being Mr. Herschel's (Spence's Essays, p. 163) under a *definite* form. In comparing these two forms of solution part by part, instead of the whole with the whole, I concluded the latter was more general than the former, whereas they are precisely equal.

Had we sought in the last example the complete solution by assuming it $= \frac{1}{2} f_1 x \cdot \varphi x$, instead, of $= \frac{1}{2} f_1 x + \Phi x$, we should have found it

$$\psi x = \left\{\tfrac{1}{2} + \varphi x - \Phi \alpha x\right\} \cdot f_1 x$$

the simplest expression I have yet seen for it.

The subsequent example I introduce more for the sake of exhibiting another method of obtaining the complete from a particular solution, than from any necessity of its testimony. Let

$$\psi x = f\left\{x, \alpha x, \ldots \alpha^{n-1} x\right\}$$

be a particular solution of any functional equation. Then, if Φ be perfectly arbitrary, and

$$f\left\{\Phi x, \Phi \alpha x, \ldots \Phi \alpha^{n-1} x\right\}$$

will satisfy the conditions of the proposed equation, it is the complete solution. For since the conditions are satisfied, and the expression contains an arbitrary function of every transformation x can have, from x to $\alpha^{n-1} x$, it must, by what we have already demonstrated, possess all the properties of the complete solution. Now a particular solution of

$$\psi x^n + \psi \alpha x^n = 1 \text{ is } \psi x = \left\{\frac{x}{x + \alpha x}\right\}^{\frac{1}{n}}$$

and therefore the complete solution is

$$\psi x = \left\{\frac{\varphi x}{\varphi x + \varphi \alpha x}\right\}^{\frac{1}{n}}$$

which coincides with the solution obtained by our *first method.* Mr. Babbage (in Phil. Trans. for 1817), has given

$$\psi x = \left\{ \frac{\varphi x - \varphi \alpha x + \varphi_1 x}{\varphi_1 x + \varphi_1 \alpha x} \right\}^{\frac{1}{n}}$$

for the solution containing two arbitrary functions ϕ, ϕ_1. It would hardly be worth while, in a thing so easy, to spend time in showing that this solution and mine are identical in point of generality; I shall therefore, as I have only exemplified in periodics of the second order, produce the testimony of a periodic of an indefinite order.

Let $\psi x = fx.\psi \alpha x$, where $\alpha^n x = x$, and the condition of possibility is $fx.f\alpha x \ldots f\alpha^{n-1} x = 1$. A particular solution of this equation is

$$\psi x = 1 + fx + fx.f\alpha x + \ldots fx \ldots f\alpha^{n-1} x$$

And if this be multiplied by ϕx the proposed equation will give $\phi x = \phi \alpha x$; whence the complete solution is

$$\psi x = \left\{1 + fx + fx.f\alpha x + \ldots\right\} \cdot \left\{\phi x + \varphi \alpha x + \ldots \varphi \alpha^{n-1} x\right\}$$

or, $$\psi x = \varphi x + fx.\phi \alpha x + fx.f\alpha x.\varphi \alpha^2 x + \ldots$$

which agrees precisely with the solution deduced from our *second method.*

Thus it is evident, so far as these testimonies go, that they confirm the truth of our conclusions, whatever be the order of periodicity; and if it were not for the troublesome operations, more complex proofs could easily be given. Hence we may, I presume, regard the difficulties respecting arbitrary functions and complete solutions, which have obscured the functional calculus of one variable with one periodic and one unknown function, as cleared, except inasmuch as they rest on the imperfections of common algebra.

It seems from what we have said, that those which have been termed *general* solutions are in fact *complete* ones. We should hence be induced to conclude that the solutions of functional equations are but of two kinds, instead of three; namely, *particular* and *complete.*

Errata in my Paper, Phil. Mag, Sept. 1824, p. 198.

Line 7, for each αx read $fx.\alpha x$
12, for $x - \alpha x$ read $x - fx.\alpha x$

XIV. *On the Velocity of Sound.* *By* Wm. Galbraith, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

SHOULD the accompanying analysis of some of the late experiments on the velocity of sound meet with your approbation, you will oblige me by giving it a place in your Journal. I am, yours &c.

Edinburgh, July 12, 1825. Wm. Galbraith.

The velocity of sound transmitted through the atmosphere has lately attracted some attention. The results deduced from experiments presented several discrepancies which it was desirable to remove. This could only be accomplished by new and varied series of experiments, under a variety of circumstances and in different climates. Among the early investigators of this problem, the effect of barometric pressure, temperature, and moisture was not sufficiently attended to, and consequently their theoretical conclusions could only be considered as rude approximations.

Newton attempted an investigation, in the scholium attached to the fiftieth Proposition of the second book of the *Principia*, and by several considerations relative to the nature and constitution of the atmosphere, arrived at the conclusion of Derham; namely, that sound moved at the rate of 1142 feet in a second of time. He immediately adds, however, that "these things will be found true in spring and autumn, when the air is rarefied by the gentle warmth of those seasons, and by that means its elastic force becomes more intense. But in winter, when the air is condensed by the cold, and its elastic force is somewhat remitted, the motion of sound will be slower in the subduplicate ratio of the density; and on the contrary, swifter in summer." Now though he did not make any estimate of the allowance for the actual state of the atmosphere, it is clear he thought it necessary; and would, without doubt, have determined it, had he been possessed of sufficient data.

As it was conceived that temperature had a considerable effect in modifying the velocity of sound, an allowance was accordingly made on that account.

Dalton first proved that the expansions of all aëriform fluids, when insulated from liquids, are uniform by the same increase of temperature, which was verified by Gay-Lussac with a more refined apparatus. They showed that 1·000 in volume at 32° Fahrenheit, became 1·375 at 212° Fahrenheit. Hence the increment of bulk for each degree is $0{\cdot}375 \div 180 = 0{\cdot}002083$

0·002083 Fahrenheit, or 0·375 ÷ 100 = 0·00375 of the centigrade scale. Proceeding on these principles, the formula frequently adopted, particularly by foreigners, is obtained, that is, $v = c\sqrt{1+0{\cdot}00375\,t}$; where v denotes the horizontal velocity, c a constant determined by observation, commonly assumed at 333·44 metres, or 1094 English feet, and t the temperature by the centigrade thermometer. Hence in this formula an allowance was made for temperature, but none for barometric pressure and moisture, which certainly ought not to be omitted, as each has its proper effect.

Newton's solution may be represented by the following formula, in which l denotes the height of the homogeneous atmosphere in feet, and g the gravitating force:

$$v = \sqrt{g \times l} \quad \ldots\ldots\ldots\ldots \quad (1)$$

But, according to the accurate theory of Laplace, Mr. Ivory in the 63rd volume of the Philosophical Magazine, p. 426, from his investigations on the astronomical refractions shows that

$$v = \sqrt{g \times l \times \tfrac{4}{3}} \quad \ldots\ldots\ldots \quad (2)$$

which agrees pretty well with experiment.

The quantity $\frac{4}{3}$, or 1·3333, has been determined by experiments. Those of Clement and Desormes give 1·3492, and those of Gay-Lussac and Welter ..... 1·3748, the mean of which is 1·362, a little greater than Mr. Ivory's estimate, and may be considered as a close approximation to the truth.

Now if in equation (2) these be substituted, observing that $l = 27818$ feet nearly, when the barometer is at 30 inches, and the thermometer at 50° Fahrenheit, and $g = 32{\cdot}2$ feet, it will become $v = \sqrt{32{\cdot}2 \times 27818 \times 1{\cdot}362} = 1104\frac{1}{2}$ feet, agreeing very well with observation. If we take the specific gravity of mercury at 13568, and that of atmospheric air at 1·22, then mercury will be about 11121·3 times heavier than air, in a mean state; or if the specific gravity of air be taken at 1·21, as there is some uncertainty, the mercury will be 11213·22 times the weight of air. In fact, a slight alteration in the specific gravity of either of these fluids makes a considerable difference in their relative weights. Let $p =$ the barometric pressure in inches, then $\frac{11213{\cdot}2\,p}{12} = l$ in feet.

$$\text{Hence } v = \sqrt{\frac{32{\cdot}2 \times 1{\cdot}362 \times 11213{\cdot}22\,p}{12}} = 202{\cdot}44\sqrt{p} \quad \ldots\ldots \quad (3)$$

If $p = 29{\cdot}7912$, the mean of Dr. Gregory's experiments, then $v = 202{\cdot}44\sqrt{29{\cdot}7912} = 1105$ feet. This agrees very nearly

nearly with his determination, which is 1107·05 at 29·79 barometer and 48°·62 Fahrenheit.

If Mr. John Farey's number 11262 representing the times mercury is heavier than air be taken, then $v = 202{\cdot}88 \sqrt{p} = 1107{\cdot}32$, agreeing almost exactly with experiment.

Now, since in general terms $v = n \sqrt{p}$, the variation of v for a change of pressure is as $\sqrt{p} - \sqrt{p'}$ ..... (4)

Supposing $p = 30$ inches, and $p' = 29$ inches, then on that account $\delta v = n (\sqrt{p} - \sqrt{p'})$, or $\delta v = n \times 0{\cdot}0921 \times \delta p$ nearly; and for one inch, $\delta v = 0{\cdot}0921\, n$. But $n = 202{\cdot}88$; therefore $\delta v = 0{\cdot}0921 \times 202{\cdot}88 = 18{\cdot}8$ feet, ...(5) for one inch of the barometer, at about its mean state.

Again, since according to the continental mathematicians $v = c \sqrt{1 + 0{\cdot}00375t}$, t being the temperature by the centigrade thermometer above zero;

This becomes by Fahr. above 32°, $v = c \sqrt{1 + 0{\cdot}002083t}$ or $v = c(1 + 0{\cdot}001042t) = c + 0{\cdot}001042\, c\, t$.

But $c = 333{\cdot}44$ metres, of 3·2809 feet each, or 1094 feet.

Hence $v = 1094 + 1{\cdot}14\, t$; and therefore the variation depending upon the change of temperature is $1{\cdot}14\, t$ .... (6)* of Fahrenheit's scale.

It is now only necessary to determine the variation for the state of the atmosphere depending upon dryness and moisture, as indicated by a hygrometer constructed upon proper principles. This cannot easily and accurately be done, as we have no experiments in which it is marked, except Mr. Goldingham's.

Making the foregoing allowances for the barometer and thermometer, which may be presumed to be nearly correct, we may ascertain what his hygrometric variation is, according to his own scale; though this is not comparable with others, for want of a knowledge of the extent and fixed points of his scale.

* This is greater than Dr. Gregory's from experiment, as may be shown thus, the barometric pressure being constant:

Ther. 27°	Bar. 29·82 inch.	Vel. 1094·2	
66	29·82	1116·1	Therefore $\frac{21{\cdot}9}{39} = 0{\cdot}561$ foot.
39	·00	21·9	

The Doctor, for want of a proper instrument, did not make any allowance for the effect of moisture; therefore this may be somewhat erroneous on that account. We hope, however, that he will resume the subject, properly provided with such an instrument.

Comparing

Comparing the experiments made in January with those in July, we have

	Bar.	Ther.	Hygr.	Velocity.		
1,	30·124	79·05	6·20	1101	$-\cdot21\times18\cdot8=-$	3·948
7,	29·914	86·65	27·85	1164	$+7\cdot6\times1\cdot14=+$	8·664
						+1101·000
	−0·210,	+7·60,	+21·65	63		1105·716
						1164·000
						58·284

Hence $\frac{58\cdot284}{21\cdot65} = 2\cdot7$ feet for one degree of Goldingham's hygrometer.

Combining two and two in this manner the twelve monthly experiments about thirty different ways most likely to produce correct results, we found 2·87 feet, from a mean of the whole, for one degree of his hygrometer*.

Introducing the foregoing principles, we may obtain a general formula for determining the velocity under any given circumstances.

If we put α for the change of velocity for a variation of one inch of the English barometer, β for that of one degree of Fahrenheit's thermometer, and γ for that of one degree of Goldingham's hygrometer, V being the true velocity required, and v that under known circumstances; then,

$$V = v + \alpha(p' - p) + \beta(t' - t) + \gamma(h' - h) \ldots\ldots (7)$$

Now from a mean of all Mr. Goldingham's experiments we have $v = 1100$ feet at 30 inches, 60° Fahrenheit, and 14° Goldingham's hygrometer. Also $\alpha = 18\cdot8$ feet, $\beta = 1\cdot14$ foot, $\gamma = 2\cdot87$ feet, $p = 30$ inches, $t = 60°$ Fahrenheit, $h = 14°$ hygrometer; and p', t', and h', the observed states of the barometer, thermometer, and hygrometer respectively.

But the velocity of sound is modified a little by the velocity and direction of the wind. Dr. Gregory observes, "that when the direction of the wind *concurs* with that of the sound, the sum of their separate velocities gives the apparent velocity of sound; and when the direction of the wind *opposes* that of the sound, the difference of the separate velocities must be taken."

* It were greatly to be desired that Mr. Goldingham would, if possible, give the means of comparing his hygrometer with some standard, or with Mr. Daniel's, which seems to have met with considerable approbation for its accuracy. This might perhaps be accomplished in some such manner as Biot has by analysis compared De Luc's and Saussure's.

We have heard that Mr. Thomas Jones, of Cockspur-street, has lately invented an improved hygrometer which may be useful in these researches.

Now

Now let φ be the angle which the direction of the wind makes with that of the sound, and ω the velocity of the wind; then $\omega \cos \varphi$ will be the correction to be applied to the velocity of sound, on account of the velocity and direction of the wind.

Hence the complete formula embracing all these corrections will be

$$V = v + \alpha(p' - p) + \beta(t' - t) + \gamma(h' - h) + \omega \cos \varphi \ldots . \quad (8)$$

or, by substituting the values previously found,

$$V = 1100 + 18{\cdot}8(p' - p) + 1{\cdot}14(t' - t) + 2{\cdot}87(h' - h) + \omega \cos \varphi \ldots (9)$$

Applying this formula to Mr. Goldingham's observations, we shall then see how it corresponds in each individual case, and be able to form some estimate of its accuracy, though there must be some slight discrepancies, on account of the velocity and direction of the wind being unknown, as well as on account of unavoidable small errors of observation.

As it is not likely he would make observations during high winds, but most frequently in moderate weather, when, according to Smeaton's estimate in the 51st volume of the Philosophical Transactions, the velocity of the wind might vary from about 4 to 8 feet per second, then the error of Mr. G.'s observations might vary on that account, from 0 to about 10 or 12 feet, according as the wind contributed to augment or diminish the apparent velocity of the sound; and this, perhaps, may be about the maximum error of the formula.

We shall now present a table embracing all these deductions.

Table of Mr. Goldingham's Experiments.

Month.	Mean Height of Barom.	Therm.	Hygro.	Observed Velocity.	Calculated Velocity.	Error.
	Inches.	°	Dry.	Feet.		
January	30·124	79·05	6·20	1101	1098·4	E 1 — 2·6
February. .	30·126	78·84	14·70	1117	1125·9	E 2 + 8·9
March	30·072	82·30	15·22	1134	1130·3	E 3 — 3·7
April.....	30·031	85·79	17·23	1145	1139·3	E 4 — 5·7
May	29·892	88·11	19·92	1151	1147·0	E 5 — 4·0
June.....	29·907	87·10	24·77	1157	1160·1	E 6 + 3·1
July.....	29·914	86·65	27·85	1164	1168·5	E 7 + 4·5
August.....	29·931	85·02	21·54	1163	1148·9	E 8 — 14·1
September	29·963	84·49	18·97	1152	1141·5	E 9 — 10·5
October ...	30·058	84·33	18·23	1128	1142·0	E 10 + 14·0
November	30·125	81·35	8·18	1101	1110·0	E 11 + 9·0
December	30·087	79·37	1·43	1099	1084·8	E 12 — 14·2
Mean	30·019	84·37	16·187	1134·33	Sum	— 40·6
At.....	30·000	60·00	14·000	1100·00		+ 39·5
Hence the negative errors exceed the positive by 1·1 foot only.					Excess	— 1·1

On considering the results obtained from this comparison, it may be noticed that the greatest deviation of the formula from observation is about 14 feet, sometimes in excess and sometimes in defect. To what cause to attribute this it is at present difficult to say.

Future observation will perhaps show how much ought to be attributed to the formula, to the error of observation, to the effects of wind, and to the variable nature and constitution of the atmosphere, which our best instruments are yet unable to detect. This is perhaps countenanced by a comparison of the September and October observations, in which the pressure, temperature, and moisture are very nearly the same; yet the velocities differ no less than 24 feet, while the results by the formula differ only about half a foot *.

This is the season at which the monsoons change their direction: and as from an examination of the plan of the grounds accompanying the observations, it may be inferred that the wind in the one case tended to augment the apparent velocity of sound, while in the other it tended to diminish it, we may reasonably conclude that this is in a great degree the cause of the discrepancy. It is the more likely, as in February and March the same circumstances occur, with nearly the same effects, though in a somewhat slighter degree.

Mr. Goldingham's allowances for pressure and moisture differ considerably from those stated here, though that for temperature is nearly the same. He makes the variation for 1° of the thermometer 1·2 foot, for 1° of the hygrometer 1·4 foot, and for $\frac{1}{10}$th of an inch of the barometer 9·2 feet! and the effect of the wind from 10 to 20 feet.

The allowance for the hygrometer is only one half of what we have made it; and that for pressure about *five times* greater! Perhaps, as the change of barometric pressure is so small in that climate, it is difficult to deduce from observation the proper allowance for it.

I conclude by expressing a hope that some experimenter, provided with all the necessary instruments to ensure the utmost possible accuracy, will undertake a series of experiments

* The paper was written thus far before the author knew at what Mr. Goldingham's allowances were estimated, the foregoing being all derived from the abstract subjoined to Dr. Gregory's paper in the Cambridge Philosophical Society's Transactions. Since that time Moll's experiments have appeared in the Philosophical Transactions of the Royal Society, embracing all the niceties of the subject in a very neat and accurate manner. They might have been susposed to supersede what I have said here on the subject; but as the paper was drawn up, and may be useful in certain cases, it may perhaps deserve a place in the Philosophical Magazine.

under

under such a variety of circumstances, as to be enabled to deduce from experiment alone the due corrections for temperature, pressure, moisture, and the direction and velocity of the wind, independent of all theory.

XV. *Thoughts on the Demonstration of certain Formulæ.* By T. S. Davies, *Esq.*

As upon the truth of the binomial theorem has been supposed to rest the truth of almost every thing we know in the higher branches of analysis, it was very natural for the most profound powers of investigation to be employed in effecting a satisfactory demonstration of it. The theorem, however, in its simplest case was discovered by *induction;* its extension was also by *induction;* and the clearest, indeed the only proof we even now possess of its truth is altogether *inductive.* On this latter topic I am aware that great names are against me: but are they not also opposed to each other? Many have attempted to perform this task, but each has easily discovered some fallacy in the other's proofs: and, indeed, I doubt whether there be any writer who has not felt a latent suspicion of the logical accuracy even of that perfect one of all—*his own.* It becomes, then, an important course of inquiry which investigates the causes of these failures; and perhaps a few very simple considerations may lead us to believe that the nature of the evidence upon which this and other theorems rest, is of a character essentially different from that which is commonly supposed to appertain to mathematical truth. It will thence appear probable that the failures have not arisen from want of skill, but from aiming at a species of proof of which the subject does not admit. Nor can the perspicuity of our processes and the facility of acquisition fail to be materially improved by a reversal of our common mode of proceeding, should that procedure be founded upon an erroneous estimate of the objects to be attained and the proper steps of attainment.

The first great principle we should ever bear in mind is, that all the "laws of number" are mere deductions from a comparison instituted between a great variety of separate cases, or else deductions from the results thus obtained. Such, indeed, is the origin of our belief of the numerical fact, that m times n is equal to n times m;

that $(x \pm y)^m = x^m \pm m x^{m-1} y + \ldots\ldots;$

that $\Delta^n \varphi x = \varphi(x + nh) - n \cdot \Phi\{x + \overline{n-1} \cdot h\} + \ldots;$

that $\phi(x+nh) = \phi x + n \cdot \Delta\phi x + \frac{n \cdot n-1}{1 \cdot 2} \cdot \Delta^2 \phi x + \ldots$;

and that $\frac{d^r x^n}{dx^r} = \frac{\Delta^n 0^n}{\Delta^{n-r} 0^{n-r}} x^{n-r}$;* and of a thousand other formulæ: indeed, of every possible formula that is true.

I do by no means intend to assert that all these propositions are equally clear and obvious to the untutored mind; but merely that they are all equally the result of a generalization of facts, which had been previously observed to be true in every one of the numerous instances which had been examined. The object of the generalization was to discover an empirical expression which should, *mutatis mutandis*, comprehend all the possible separate cases; and we felt justified in the use of the rule thus formed, so long as no exception to its generality was discovered, whilst from every instance of its accordance with separate cases that came under our notice we felt it invested with an additional degree of certainty. This was asserted in the binomial, theorem; and by the analogy in integer numbers between that and some of the differential theorems, the same generalization was suggested in this class of theorems which had been successfully tried in the binomial. Such an extension was very obvious; but it required us to generalize (if, indeed, that could be done) the signification of the index of the characteristic, so as to include fractions, irrationals, and imaginaries, in order to render the resulting expressions *intelligible*. This being done, it became easy to verify the theorem by application to particular cases, and thus to place it amongst the admitted principles of the science.

My intention in the present paper, however, was principally to notice Mr. Herapath's Demonstration of the Binomial Theorem (Phil. Mag. May 1825), and his extension of it to the development of any function in terms of the finite differences of that quantity; and to point out where that gentleman has deceived himself by an appearance of greater rigour than he had actually obtained in his demonstration. I shall also notice, *en passant*, that given by the Rev. Arthur Brown in his "View of the First Principles of the Differential Calculus," p. 59, Cambr. 1824.

In the first place, upon what authority but that of induction does Mr. Herapath assume the *n*th line in his class of equations (B), p. 323? The induction, I grant, is easy, and the law tolerably obvious: but it is induction still, and the principle of generalization is essentially involved. The formula has been found to hold true in all the integer numbers that *have*

* Mr. Herapath, Phil. Mag. May 1825, vol. lxv. p. 327.

been

been tried, and we thence infer that it will hold for all that *can* be tried.

Again, Mr. Herapath finds that with a little contrivance, without the admission of principles essentially new, he can show its application to $(x+y)^{r+v}$ when $r+v=n$, and r, v, and n are whole numbers. So far, viewed as a series of simple inductions, and resting on the evidence of numerous trials, the results are satisfactory.

Here, however, Mr. Herapath is obliged to pause, and to call in the aid of a new principle on which to found his inferences; viz.—*that because it was true when* r *and* v *were integers, it must be true when they are fractions, and even when they are irrationals and imaginaries too!*—the only condition to which these fractions, irrationals, and imaginaries are subjected being, that their aggregate shall be an integer. From this again, he discovers that because it is true of *two* such quantities, it must also be true of *one;* and hence that it is true universally.

Had the former steps been demonstrated in the usually strict meaning of the term, still it must be admitted that this latter step is altogether gratuitous and unwarranted; perfectly *outré* to the spirit of that pure logic of which the mathematical sciences are esteemed the very essence. The investigation lays it down as an essential datum that r and v shall be *integers;* and immediately proceeds upon the supposition of their being *fractions*, *irrationals*, or *imaginaries*, taken at pleasure! I admire the mind that can readily generalize particular results; it is always a mind of great capability: but it is asking from us too much to require our assent to a proposition which rests upon the authority of a demonstration like this. Why not at once assume n as the representative of any number whatever, even an imaginary one (if that does not involve some absurdity), rather than adopt the tedious process of considering r and v as both fractions? It was thus that Newton proceeded, and he verified the formula by numerous trials. Others since his time have involved the theorem in a maze of symbols, from which they have dexterously evolved a medley of conclusions, which they call demonstrations: but, so far as the proof of the theorem is concerned, the matter rests just where he left it, save that it has received numberless numerical verifications in its passage through the hands of successive computists.

The "*demonstration*" given by Mr. Herapath, of

$$\Delta^n x^v = (x+n)^v - n(x+n1)^v + \ldots, \text{ p. 326,}$$

also fails, in consequence of his failure in the demonstration of the binomial theorem: and against that gentleman's reasoning

ing upon periodical functions (Phil. Mag. November 1824, p. 333) where the indices are fractional, we may urge the same objections as we have done against his reasoning upon the binomial. Those assertions *may* be true—and they probably *are* true: but no proof of its truth has yet been given; and I much mistake the nature of demonstration, if any proof but that derived from successive inferences can ever be established.

The demonstration given by the Rev. Arthur Brown, to which I before referred, proceeds in the usual manner when n is a positive integer. This integer he next supposes converted into an *equivalent vulgar fraction* $\left(\frac{m}{n}\right)$, and argues that it still is true.—Granted. But he proceeds, "*Since this is true when* $\left(\frac{m}{n}\right)$ *is a* WHOLE NUMBER, *it will* ALSO *be true when* $\left(\frac{m}{n}\right)$ *is a* FRACTION! (View, p. 60.) This is the work in which, after an unsparing review of the state of academic mathematics, the author has "endeavoured to give a *rigid* demonstration of every proposition!" Pref. xxi.

Some years ago the views entertained by Mr. Herapath occurred also to myself: though of the "more general calculus" to which he refers (but of which he so carefully conceals all traces) I am unable to form an opinion. My investigations were conducted on the assumed truth of the differential formulæ when extended to any form whatever of n: and I had examined most of the functions noticed by Mr. H., together with many important ones to which he has not made any reference, though doubtless they have more or less attracted his attention. A concurrence of circumstances—amongst which the annunciation of Mr. Herapath's discoveries more than two years ago in the Phil. Mag.; the difficulty of satisfying myself at that time concerning the fundamental principles of analytical certainty; and the force of those claims upon my attention, which I considered paramount even to the claims of science;—these prevented me from pursuing the inquiry so far as under more auspicious circumstances I probably should have done: and from knowing that the question was likely to be fully investigated by a mind of the vigorous constitution of Mr. Herapath's, and with the aid of a new and more extensive calculus, I felt altogether unanxious concerning the state of my own investigations. It will be scarcely necessary, then, to disclaim all hostile feelings in the remarks which I have made; and to assure that gentleman, that, but for the error into which I conceive he has fallen, and the consequent loss of much valuable time in vainly attempting impossibilities,

ties, I should not have appeared before the public on this occasion.

Mr. Herschel was aware that to deduce the differential formulæ in the usual way had very much the appearance of "an inductive process:" and to evade the difficulty he resorts to a method equally inductive—"the calculus of generating functions *." In the proof by this method, were no other inductive principle involved, there is yet the binomial theorem. But there are other inductions mingled up in the investigation: for instance, the assumption of a *series* to represent the development of ϕt; which series, especially as following a regular and successively dependent law, we do not know to be always possible. It is true Mr. Herschel attempts to parry this objection, by assuming the series as the "*definition*" of the function ϕt: but even in this view, except considered as induction, it is inadmissible; for how can we define any function, except by stating some constantly observed quality or circumstance belonging to it, and which may serve as a distinguishing character of that function? In any other case than this it is not a definition, but an assumed property; and our assumption is therefore merely a fiction, to be subjected to investigation and analysis,—not less so than any theorem in the ancient geometry; and by comparing its results with previously established principles, we learn its consistency with those principles. If correspondent, we admit the fiction; if not, we reject it. Here, however, since this series is considered a general representation of every numerical case of every possible series, we can only compare it with each separately, and admit its *partial* truth by the successive probabilities which arise out of those comparisons. Upon what then, but induction, does the principle of investigation itself depend? We have found *in all the cases we have yet tried*, that ϕt may be developed in the form of the fundamental equation of the calculus of generating functions: and we cannot question the *probability*, though we must ever make a reserve of the term "*absolute certainty*," that every possible form of ϕ is such as would admit of such an evolution. Indeed, Mr. Herschel's view of $\phi t = \ldots$ series $\ldots$ being a definition, is at once resolvable into the principle which La Grange calls the "*theory of series:*" and this principle is merely an observation that certain properties belonged to every separate function which had been examined, and an induction of the universality of a law, in consequence of no deviation from it having yet been noticed.

Taking this view of the subject, there appears no reason for Mr. Herapath's considering the expression for the develop-

* *App. Transl. Lacroix Diff. et Int. Calc.* p. 472.

ment

ment of $\Delta^{n\sqrt{-1}}$ as *demonstrated;* nor, on the other hand, for Mr. Herschel's denominating the asserted equality of the function and its development "*a definition**." That the theorem is true, provided the expression can be considered *intelligible*, there is a high degree of probability; but that probability can only arise from the identity of its general form of expansion with the general form of expansion of the binomial theorem. Indeed, I cannot doubt, on the faith of the general principle, the truth of

$$\left\{\varepsilon^{\frac{d}{dx}}-1\right\}^n=\Delta^n,$$

in their developments, whatever be the form assigned to n: against the affected rigour and consequent inconclusiveness of its demonstrations rest the only objections I have to make.

To conclude, and in reference to notation, I shall just remark that Mr. Babbage's views† on this subject do not appear to be quite correct, though the phraseology which he generally uses indicates a near approximation to such correctness; perhaps a nearer approximation than can be found in the writings of any other mathematician whatever. However, concerning new notations, "not to agree with, but to *include* the former," we ought to remark that this including notation is merely a statement of facts (or in truth, a more general theorem), so contrived as to embrace in one expression the facts of two or more of the old notations; just in the same manner that n is used to include 2, 3, 4, &c. as indices in any power of φx. It is true, that from this new statement, or notation, new relations might be observed, or new theorems suggested, or the relations so expressed might excite some collateral inquiries arising out of mere analogy, as in the differential theorems to which I have so often referred, and which were suggested by the binomial. The formulæ were not intended originally to "*include*" negative, fractional, irrational, or imaginary values of n; the signification of the formulæ was so interpreted *after* experiments had shown that the formulæ held good in every tried case of all those classes of values. Improvements in notation are in truth identical with the discovery of new theorems, or with a more extended application of the old.—But I have far exceeded the limits which I had prescribed to myself (notwithstanding several important considerations on these subjects remain untouched, and not even hinted at): I shall, therefore, defer the further inquiries to which the considerations already stated so obviously lead, till a future and more convenient period.

Bristol, July 19, 1825. T. S. D.

* I infer that it is to Mr. Herschel that Mr. Herapath alludes, Phil. Mag. May 1825, p. 326.

† Cambridge Phil. Trans. vol. i. pt. i.; and Edinb. Encycl. art. "Notation."

XVI. *Che-*

XVI. *Chemical Examination of some Antiquities found in the Vicinity of Brool, on the Rhine. By Dr.* Rudolph Brandes.

THE importance of the vicinity of Brool to the antiquarian is sufficiently known, as well as the great discoveries made there by the diligence of M. Dorow. It was through his kindness that I obtained the antiquities considered in this memoir, with the request to analyse them as speedily as possible; a request, however, which I have not been able to fulfill until now.

I. *A Fragment of Roman Glass found near Brool.*

The invention of glass is known to be very ancient; nevertheless few antique remains of it have come down to us, or have been analysed.

Although the art of manufacturing glass was not carried to that degree of perfection among the ancients to which it has been brought in our days, still in some branches of it they had gone very far, as has been sufficiently shown by the learned investigations of Winkelmann.

The piece of glass which I obtained from M. Dorow was a fragment of a round vase, and weighed about 10 grains. Its colour was of a milky-white with a very blueish cast. A pellicle of a brilliant gold-colour covered its exterior, and in part its interior surface. This had so much the appearance of gilding, that without a chemical trial one would have taken it to be gold; but I shall show below that this was not the case. The long period of time during which the glass had been exposed to the effects of the air, water, and the pressure of the earth, had made a visible impression on it; so much so, that it was in a mouldering state, had entirely lost its firmness and brittleness, and when broken, pressed, or scraped, fell into small leaves like mica. It had completely lost its transparency; but it was still evident, from its appearance in the centre, that it was originally perfectly transparent, that part, from having somewhat resisted the destructive effects that had acted upon the rest, being so still. Wherever the glass was covered with the gold-like pellicle it was not transparent, but where free from it, it was perfectly clear.

By endeavouring to separate that covering, no gold-leaf was detached, but thin leaves of glass; and the surface beneath soon offered a similar appearance. In some places that metallic tarnish assumed a fine blueish, red, or green hue; and a similar appearance was produced by taking off the apparently

* From Schweigger's *Journal*, Band x. p. 304.

metallic pellicle which was on the inside. This shows that the cause of this tarnish was the same as that which acts upon glass long exposed to the weather,—such as in old church windows for instance; and which has a similar appearance. However, to convince myself completely of the absence of gold, I heated as many as possible of the shining glass leaves in nitric acid, by which process the gold-coloured covering entirely disappeared, and the leaves remained without colour.

In order to find out the component parts of the glass, I submitted it to the following process:

a) A portion of the glass was reduced to a powder and boiled in nitric acid, the liquor filtered off and saturated with ammonia, which threw down a precipitate that was again dissolved in nitric acid, and which gave no precipitate with sulphuretted hydrogen. Nor did sulphuric or muriatic acid produce any turbidity in it: but when that part of the solution which had been treated with sulphuretted hydrogen was saturated with ammonia, a blackish-green precipitate was produced, which, after being again boiled with nitric acid and mixed with ammonia, gave a whitish precipitate, which became browner by exposure to the air. This was again dissolved in nitric acid, the product neutralized and mixed with benzoate of potash, which gave a precipitate of a brownish-white: a whitish cloud was produced in the liquid filtered from it, by means of carbonate of potash.

This precipitate, then, must have consisted of the oxides of iron and manganese. After some re-actions, it seemed as if some traces of arsenic were still left in the nitric solution: however, the indications were too undecided to give this with certainty.

b) The ammoniacal liquor of (*a*) being evaporated to dryness, the remaining substance was dissolved in dilute nitric acid. In this solution, sulphuretted hydrogen gave a dark-brown precipitate of sulphuret of lead.

c) The precipitate not dissolved by nitric acid was ignited with nitrate of barytes in a crucible of platinum, the remaining mass dissolved in water acidulated with nitric acid, evaporated to dryness, and the residuum extracted by acidulated water. This consisted of pure silica.

d) The acid solution (*c*) gave with ammonia a slight brown flocculent precipitate, from which caustic potash extracted a trace of alumina; and the rest was similar to the mixture of iron and manganese in (*a*).

e) The solutions treated with ammonia were freed from barytes by means of nitric acid, evaporated and ignited. The small residuum that was left, was dissolved and crystallized

on

on a glass dish, and it proved to be sulphate of soda with a small quantity of sulphate of lime, without any salts of potash.

The result of this is, that the glass consists of

1. Silica
2. Soda
3. Lead
4. Oxide of manganese
5. Oxide of iron
6. Lime
7. Alumina.

Of these constituents the silica formed about two thirds, and the other substance the remaining third of the whole mass.

II. *Sealing-wax.*

A piece of a light brown-red waxy substance appeared to be a fragment of sealing-wax of which the Romans had made use; for the following investigation seems to favour such an hypothesis.

The piece weighed about 20 grains, became soft and fluid when heated, burned with a flame, left a carbonized residuum, and by a greater and continued heat a very small yellowish hard substance.

a) A part of the substance was rubbed small, and the reddish powder brought into contact with cold æther. This was poured off the undissolved part, the æther evaporated, and a sediment of a bright yellow colour obtained thereby, which was acted upon by cold alcohol, and left a deposit of a small particle of wax; and the spirituous solution gave by evaporation a mass very tough when warm and more stiff when cold, which might be drawn into long threads, was colourless and transparent, and most like turpentine or a similar balsam, although nearly without smell.

b) The residue left after the action of the æther was now boiled in alcohol, and thereby nearly all dissolved: this solution became on cooling very dull, and deposited a large quantity of a white sediment possessing all the qualities of wax. The solution left on evaporation some wax and a small quantity of a gummy substance.

c) The residue which had not been dissolved by this treatment was not taken up even by boiling water: it was still of a reddish colour. By the addition of a few drops of nitric acid it disappeared in part. The nitric solution assumed a whitish colour by the addition of ammonia, and gave with sulphuretted hydrogen a black precipitate. The nitric acid had dissolved lead, which had been probably added as *colouring-matter* to the sealing-wax.

d) The residue of (*c*) was still of a brownish colour, was partly dissolved by alcohol and caustic potash, and was probably a yellow gum. When heated it became liquid, and was burned up, with the exception of a small residuum.

e) The residue treated with potash in (*d*) had left some traces of a reddish powder and of a few shining metallic leaves, of the appearance of mica. This residue was heated with three drops of nitric acid and a little water; by which the reddish powder was dissolved, but the metallic leaves still remained. The solution of nitric acid was poured off, neutralized with a little ammonia, and a black turbidity was imparted to it by a current of sulphuretted hydrogen. It is therefore probable that the reddish powder was a little more red-lead. The remaining leaves of metal were now treated with one drop of nitric acid and two drops of muriatic acid, by which they were completely dissolved: we must consider them therefore as gold-leaf, but in so small a quantity that it was impossible to try any further experiment on them.

From the foregoing examination it will appear that the sealing-wax consisted for the most part of common wax, to which a little gum and turpentine had been added, and which seemed to have been coloured principally by red-lead; containing besides a few leaves of gold, which seemed to have been added to enhance the beauty of the wax, although in very small proportion.

XVII. *On Metallic Titanium. By Dr.* Walchner *of Friburg, in the Breisgau**.

M. MUNZING of this place gave me lately several specimens of iron from the foundries in the highlands of Baden. Among these were, on a piece of slag from the bottom of a furnace, several small cubic crystals of a middle tint between gold-yellow and copper-red, and with a strong metallic lustre. Some of them exhibited striæ parallel to the sides of the cube, and seemed to be of a darker hue. They were very hard, scratching glass very strongly, and rock-crystal visibly; but at the same time so brittle that the blow of a hammer reduced them to powder. Owing to the very small quantity in my possession, I could not determine their specific gravity. Heated separately by the blowpipe on charcoal, they could not be melted; but they lost their brilliancy by it, and became brown to some distance from the part acted upon. They also remained undissolved in glass of borax. Salt of phosphorus, by a continued heat at the point of the interior flame, dissolved a small quantity. After treatment in the reducing flame, the glass on cooling assumed a slight amethyst-colour.

* From Schweigger's *Journal*, Band xi. p. 80.

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This reaction became more apparent when the melted pearl was touched with nitre. Acids had no effect on these crystals; and even aqua regia, with which they were frequently treated, left them untouched. It merely extracted the particles of iron attached to them, whilst the crystals preserved their form, lustre, and colour unchanged. On being melted with nitre, they were oxidated. I took advantage of this circumstance, in order to procure a solution of the oxide, by melting a small quantity of the crystals (which had been previously freed as much as possible from every particle of iron by means of aqua regia) with nitre, borax, and a little soda, and dissolving the mass in muriatic acid. The solution was colourless, and deposited, whilst boiling, white flakes. Caustic and carbonated alkalies gave a white precipitate; triple prussiate of potash with a little excess of acid, a dirty-green precipitate, which, when ammonia was poured over it, became white; tincture of galls, a copious precipitate of a dark orange-colour; oxalic acid, phosphoric acid, arseniate of potash, white precipitates; metallic zinc produced blueish-black flakes, which, on freeing them from the air-bubbles with which they were covered, became white. Hydrosulphuret of potash gave a bottle-green precipitate.

All this evidently proves that the cubes described are *metallic titanium*, and identically the same with those first described by Wollaston*. Their conducting power for electricity I could not investigate, on account of the smallness of the crystals.

The piece of slag on which the crystals occur between small globules of raw iron, is from the High furnace of Kandern, in which pea-iron-ore is smelted. Nothing similar had ever been found before. I was curious to ascertain the origin of the titanium, and examined the pea-iron-ore for it. One attempt before the blowpipe made me aware of its presence; but its quantity must be very minute, since a very small bead was formed of oxide of titanium and salt of phosphorus. Probably the crystals were formed by the reduction of the oxide of titanium contained in the pea-iron-ore, by means of the high temperature of the above furnace; and there is no doubt but that on a closer examination of the produce of that establishment they will be found frequently.

* See Phil. Mag. vol. lxii. p. 18, and vol. lxiii. p. 15.

XVIII. *On*

XVIII. *On Mr.* Herapath's *Demonstration of the Binomial Theorem.*

[We have received a communication occasioned by the letter of a Correspondent in our last Number, respecting Mr. Ward's objection to Mr. Herapath's demonstration of the Binomial Theorem; from which we give the following extract.—Edit.]

"A GENTLEMAN who, in your last (p. 50), signs himself 'A Correspondent,' appears to be highly offended at Mr. Ward, for having (vol. lxv. p. 432) noticed and supplied a defect in Mr. Herapath's demonstration of the Binomial Theorem.—Now, sir, in justice to Mr. Ward, I assert that the part of the demonstration which he has supplied is the most important part (in the demonstration for integral exponents), and *that* chiefly from which the proof can be fairly inferred."

"Further, since a product implies the multiplication of *two* or more factors, that quantity ought not to be denominated a product which contains but *one* factor: hence, in strict propriety, q must be *greater* than Q; and hence, even in this respect, your correspondent has no reason to accuse Mr. W. of being *too hasty*. "I am, sir, &c.

Wisbeach, Aug. 2d, 1825. Y+Z."

XIX. *Notices respecting New Books.*

Zoological Researches in Java and the neighbouring Islands. By Thomas Horsfield, M.D. F.L. & G.S. London, 1821—1824. 4to.

[Continued from p. 63.]

TO the birds described in the concluding three numbers of this work, we shall add those considered in the numbers formerly noticed in the Philosophical Magazine, for they include several interesting new genera, of which the names only were given upon that occasion; and no analysis of the work has yet appeared in any other publication. We shall enumerate the whole, in the order they take according to Mr. Vigors's development of the Quinary Distribution, in this department of nature. It will likewise be useful to refer to some recent memoirs by that naturalist, showing in detail the existence of the same principle in some of the subordinate subdivisions of the class.

Commencing then with the Raptores, we find three species of one of the normal families, (the *Falconidæ*,) described in this work; viz. *Falco Ichthyætus*, *F. cærulescens*, and *F. Limnæetus*.

F. Ichthyætus. F. fuscus, cerâ cæruleâ, pedibus flavescentibus,

tibus, ventre posticè crisso cruribus caudâque præter apicem albis, capite canescente.—*Jokowuru*, of the Javanese.

This species belongs to the *Stirps Aquilina* of Mr. Vigors's distribution of the *Falconidæ*, and is referred by him to the genus *Pandion* of Savigny, of which the Osprey, *F. Haliæetus* Linn., is the type. It presents, however, a strong approximation to *Haliæetus*, the next genus of Fishing Eagles, and stands osculant between the two groups*. Dr. Horsfield gives the following interesting account of its habits.

"The *Falco Ichthyætus* is found in Java, always near lakes or on the banks of large rivers. It is by no means generally distributed. I met with it in two situations only: one of these was on the banks of the largest river of the eastern parts of the island, the river of Kediri. Here it was very numerous, and I obtained most of the specimens which I afterwards brought to England. I frequently surprised it, perched on the extremity of a dead branch, in the attitude in which it is represented in the figure which illustrates this article, watching its prey. But I had previously become acquainted with its appearance and manners. During my residence on the hills of Prowota, situated about twenty miles south-east of Semarang, I found a single pair of these birds, which had established themselves for several successive years, near a village on the confines of an extensive lake formed by the annual accumulation of water during the period of rains, and distinguished by the name of Rawa (lake) of Damak. The birds had built their nest on the summit of a very large tree, where their motions were often watched by the natives; and by the assistance of one of these, I obtained both the male and the female bird, and examined their nest. The latter was constructed in a rude manner, of branches of trees. The branches which were placed on the exterior were more than an inch in diameter; the inside was lined with small twigs: it was irregularly round, and very slightly excavated. The birds were surprised during the period of incubation. The male was procured living, and kept in confinement several weeks; the female was unfortunately strangled in the snare which was set for it. The nest contained one young bird, recently hatched, and a single additional egg, which being placed under a hen was hatched in twenty-four hours. The male bird, on being caught in the snare, permitted itself to be seized by the native who ascended the tree, without making any resistance. It was brought to me without delay. The bird at this time lay in the arms of the native, apparently con-

* Sketches in Ornithology. Zoological Journal, vol. i. p. 321.

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scious of its situation, and without making use of its claws or bill, or exerting any efforts to extricate itself. It suffered itself to be handled and examined very patiently. Being placed in a large cage, I had an opportunity of observing it during several weeks. It made a few efforts in the beginning to set itself at liberty; but finding them ineffectual, it soon, by its calmness and dignity of manners, exhibited a resemblance to the more noble species of Falcon. It stood quiet in its cage, regarding every thing that passed with a steady look; if a person approached very near, it retired gradually, without showing any alarm. During the first two days of its confinement our bird refused food altogether, although it was plentifully supplied with fish. After the expiration of this time, it opened its bill when a fish was offered to it; and although very hungry, it seemed to distrust this unaccustomed mode of taking food. A small fish being now carefully placed in the bill, without any resistance on the part of the bird, was permitted to remain a considerable time, and after much hesitation was finally swallowed with great caution. By mild treatment the bird gradually became accustomed to this mode of taking food, and after several days it had acquired sufficient confidence to take a fish from the hand of any person that offered it. Its usual attitude was with the bill half opened. The bird appeared to have frequent thirst, and took up copious draughts of water with its bill.

"The pair of birds which I here obtained had been carefully observed by the natives of the village of Brambatan for several years. Their cry resembled that of the Osprey. They lived exclusively on fishes, which they obtained abundantly out of the lake above mentioned. They never attacked fowls or other animals. They daily resorted to the lake, over which, suspended in the air or sailing slowly along, they watched their prey: on observing a fish, they darted on it with impetuous velocity. More rarely they pursued a more passive conduct, and watched the lake from the trees in its neighbourhood. The nest of this pair of birds had before my arrival been repeatedly visited by the natives. The female was observed to lay two eggs at a time, and the young birds were driven from the neighbourhood as soon as they were able to provide for themselves."

F. cærulescens. F. nigro-cærulescens subtus ferrugineus, hypochondriis tibiis posticè plagâque laterali colli atris, remigibus rectricibusque intus albo fasciatis.—*Allap*, or *Allap-allap*, of the Javanese. *F. cærulescens* Linn. &c.

This beautiful species, the smallest of its race, was first described by Edwards, in his Natural History of Birds, published

lished in the year 1750, from a specimen forwarded to Dr. Mead, from Bengal. It belongs to the Falconine stirps of the family, but is closely allied to the genus *Harpagus* Vigors, belonging to the preceding Accipitrine group, by the double tooth on its upper mandible, and its short wings. Mr. Vigors has erected it into a genus, by the name of *Ieraæ*, applied by Aristotle to the greater part of the short-billed *Falconidæ**.

"The *Falco cærulescens*, as it occurs in Java," says Dr. Horsfield, "has presented to me the following observations. —It has a very robust fabric. The bill has a double notch, which is deeper than in any other species belonging to the '*Falcones propriè sic dicti*' of Bechstein that I have examined. The entire length is six inches and a half. Upper parts blueish-black and glossy. Throat, breast, axillæ, sides of the neck, forehead, and a line continued from the environs of the bill over the eye and along the neck, white, with a ferruginous tint. Lower part of the breast, abdomen, vent, and thighs ferruginous. Hypochondria, thighs posteriorly, and a broad patch extending from the eye along the side of the head, black; the plumes which cover the thighs behind are terminated by long silky filaments, or radii, which are straggling and pendulous, and by their laxity and irregularity afford a peculiar character to our bird. This is not noticed by Edwards in the description of the specimen which he delineated. From the General Zoology it appears that in one specimen, which was considered a male bird, the under parts agreed with the bird as found in Java, excepting the colour and distribution of the plumes just mentioned. In the bird figured by Edwards the under parts were bright orange-yellow, and it exceeded our bird nearly one inch in length.

"I became acquainted with the *Falco cærulescens* in the year 1806, in the eastern districts of Java: here I obtained a single individual. I had no opportunity of observing its manners personally, but the natives described it as uncommonly bold in the pursuit of small birds. During the latter period of my abode at Surakarta, several individuals were brought to me from the range of southern hills, which are covered with forests. In other parts of the island I have not observed it."

F. Limnæetus. F. fuscus, caudâ subtus præter apicem albido-cinereâ, tarsis usque ad extremitatem densè plumosis. —*Wuru-rawa*, of the Javanese.

This species, first described by Dr. Horsfield in his Systematic Arrangement of Birds from Java, published in the Linnean Transactions, is peculiarly distinguished by the

* Zool. Journ. vol. i. p. 328.

smallness of the bill and toes, and the plumose covering of the *tarsi*. M. Temminck has expressed a suspicion of its being identical with his *F. niveus;* but Dr. H. states his conviction that these species are distinct. According to Mr. Vigors they both belong to that group of *short-winged Eagles* which is distinguished by a rather feeble and elongated bill, and slender, lengthened *tarsi*, feathered to the toes; and which also includes *F. atricapillus* of Cuvier. All these birds, as stated by Mr. V., appear to be strongly allied, if not to appertain, to the genus *Morphnus* Cuv.* It may perhaps admit of discussion whether the piscatory habits of the present species will not place it nearer the *Fishing Eagles*.

The only remaining bird described in this work which is decidedly of a raptorial character, is

Strix badia. S. badia nigro punctata, subtus pallidior, gulâ juguloque albidis, torque fusco.—*Wowo-wiwi*, or *Kalong-wiwi*, of the Javanese.

Eight species of *Strigidæ* have been discovered in Java by Dr. Horsfield; three belonging to the division of the genus *Strix*, comprising the *Eared-Owls*, and five to that consisting of the Owls with *smooth heads*. Of the latter the *S. javanica* of Gmelin, shown by Dr. H.'s specimens to be merely a variety of the *S. flammea* of Linnæus, is the most generally distributed. The present species is one of the rarest. It never visits the villages, but resides in the closest forests, which are the usual resort of the Tiger. The natives even assert that the *Wowo-wiwi* approaches that animal with the same familiarity with which the Jallak (*Pastor Jalla* H.) approaches the Buffalo, and that it has no dread of alighting on the Tiger's back.

The next bird we have to notice is a species of *Podargus*, an extraordinary genus established by Cuvier, and shown by Mr. Vigors to be the immediate passage from the *Birds of Prey* to the *Perchers;* its bill combining the different forms of that of the genera *Strix* and *Caprimulgus*, and its legs, though still retaining the characteristics of the latter, related to those of the former by their superior robustness:—*P. Javanensis*. P. rufescente-isabellinus fusco-pulverulentus, caudâ undulato-fasciatâ.—*Chaba-wonno*, of the Javanese. Its habits are retired and nocturnal.

Of the birds belonging to the remaining orders, though many of them are highly interesting, we have room only to give the characters; arranging them, however, according to the natural order developed by Mr. Vigors.

* Zool. Journ. vol. i. p. 325.

Ordo

Ordo Insessores, Vigors. Tribus Fissirostres. Fam. Todidæ.

Eurylaimus. Char. essent.—*Rostrum* capite brevius, validum, depressum, basi posticè dilatatum. *Rictus* amplissimus. *Maxilla* culmine obsoleto, apice adunco, emarginato, tomiis verticalibus. *Nares* basales, subrotundæ, apertæ, nudæ. *Pedes* gressorii. *Digiti* compressi, medius fere longitudine tarsi. *Alæ* caudâ breviores. *Rectrices* 12.

E. Javanicus. E. capite toto corporeque subtus vinaceis, dorso alisque perfuscis flavo variis, caudâ atrâ fasciâ subterminali albâ.

This genus unites the *Todidæ* with the *Caprimulgidæ.*

Calyptomena viridis. C. viridis nitens, macula utrinque ad latus nuchæ fasciis alarum tribus obliquis remigibusque præter marginem exteriorem atris.—*Burong Tampo Pinang*, of the Malays. Discovered by Sir Stamford Raffles in the retired parts of the forests of Singapore, and the interior of Sumatra.

Fam. Halcyonidæ.

Dácelo pulchella. D. supra thalassino atro alboque fasciata, capite badio saturato, vertice occipiteque azureis, gulâ jugulo-que albidis, abdomine ferrugineo diluto.—*Tengke-watu*, of the Javanese. Found once only by Dr. Horsfield, in a low range of hills called the Hills of Prowoto, about twenty miles south-east of Semarang.

Alcedo Biru. A. subazurea nitore thalassino, remigibus internè caudâque subtus fuscis, gulâ jugulo abdomine maculâ colli laterali alisque subtus albis.—*Meninting-watu*, or *Burung-Biru*, of the Javanese. Common in Java.

Tribus Dentirostres. Fam. Muscicapidæ.

Muscicapa Banyumas. M. supra saturato cyaneo nigricans, subtus badio rufescens, lineâ frontali alarumque flexurâ azureis, maculâ laterali capitis collique aterrimâ anticè orbitas loraque complectente ad rostri basin extensâ posticè attenuatâ, remigibus rectricibusque nigris.—*Chiching-goleng*, of the Javanese, in the province of Banyumas, where alone it occurs; *Muscicapa cantatrix* of Temminck; Banyumas Fly-catcher of Latham.

M. hirundinacea. M. viridi-chalybeo nigra, subtus uropygioque albis.—Obscure Fly-catcher of Latham; *M. hirundinacea* Reinw. Rarely observed in the central parts of Java.

M. Indigo. M. obscurè cæsia, remigibus rectricibusque nigris, his basi axillis ventre crissoque albidis. *Nil-nilan*, of the Javanese. Lives solitarily on the most elevated mountain-peaks of Java.

Fam. Laniadæ. Sub-fam. Edolianæ, Swains.*

Irena. Char. gen.—*Rostrum* mediocre, cultratum; maxilla apice adunca emarginata. *Culmen* arcuatum, elevatum, inter nares carinatum, utrinque a basi ultra medium usque sulco obsoleto exaratum, lateribus subconvexis. *Nares* basales subrotundæ, vibrissis rigidis plumisque velutinis obtectæ. *Alæ* caudâ breviores. *Remiges:* 2—6 externè emarginatæ, 3—6 longiores subæquales; secunda sequente abruptè brevior, prima subspuria. *Cauda* mediocris, truncata. *Tarsi* digitique breves. *Acropodia* scutulata. *Ungues* parvi, fortius curvati, debiles.

Irena puella. I. atra, corpore colloque supra cervice tectricibusque alarum primis, caudæque superioribus et inferioribus cyaneis, nitore saturato azureo. *Bressi*, of the Javanese; *Biang-kapoor*, of the Sumatran Malays. Found in Java, Sumatra, and the adjacent islands.

Fam. Merulidæ.

Oriolus xanthonotus. O. ater, pectore et ventre albidis nigro striatis, dorso uropygio scapulis axillis crisso rectricibusque internè flavis, rostro rubescente, pedibus nigris. *Attat*, of the Javanese. Found solitary in a few circumscribed situations in Java.

Turdus varius. T. supra castaneo-testaceus, pennis apice atro-fuscis, subtus albidus nigro fasciatus, pectore fasciâ unâ hypochondriis fasciis pluribus latioribus, remigibus margine rectricibus extimis totis testaceis, vertice obsolete-cristato. *Ayam-ayaman*, of the Javanese. Inhabits the thick forests which cover the mountain Prahu.

T. cyaneus. T. niger, nitore cyaneo saturatissimo. *Arreng-arrengan*, of the Javanese. Lives retired in the closest forests, and is very rarely observed.

Timalia. Char. essent.—*Rostrum* validum, compressum, altum. *Maxilla* arcuata, lateribus planis, culmine amplo rotundato levissime emarginato. *Mandibula* robusta. *Nares* subrotundæ in foveâ sitæ. *Alæ* breves rotundatæ. *Cauda* elongata gradata. *Pedes* validi. *Unguis* posticus medio antico duplo major.

T. pileata. T. subolivaceo-fusca, pileo castaneo, gulâ jugulo que albis nigro lineatis, abdomine sordidè testaceo. *Dawit*, or *Gogo-stite*, of the Javanese.—This species is not unfrequent in the groves and small woods which abound in every part of Java. "It often approaches the villages and plantations, and constructs its nests in the hedges; it is one of the social birds which delight to dwell in the neighbourhood of cultivation. In

* Zool. Journ. vol. i. p. 305.

large

large forests I have not observed it. Its flight is low and interrupted. Wherever it resides, it is a welcome neighbour, in consequence of the peculiarity and pleasantness of its note. This consists of a slow repetition of the five tones of the diatonic scale (C D E F G), which it chants with perfect regularity, several times in succession, and at small intervals of time. The sixth tone is sometimes added; but as this requires apparently an extraordinary effort, it is by no means so agreeable to a musical ear as the simple repetition of five notes, which appears to be the natural compass of the organs of the bird*."

T. gularis. T. supra fusca remigibus rectricibusque saturatioribus, subtus flavescens lateribus olivaceis, gulâ pectoreque luteis lineis sagittatis notatis. *Burung-Puding*, of the inhabitants of Sumatra. *Motacilla gularis* Sir S. Raffles.

Fam. Sylviadæ.

Iöra. Char. essent.—*Rostrum* mediocre, rectum, validiusculum, æqualiter attenuatum, culmine rotundato. *Mandibula* robusta. *Tomia* dilatata, recta, acuta, diaphana, maxillaria emarginata. *Alæ* breves, obtusæ. *Cauda* mediocris, subrotundata.

I scapularis. I. olivaceo-viridi-flava, remigibus nigricantibus externè flavido internè albo-marginatis, alis maculis albis confluentibus scapulis parallelis notatis, abdomine pectoreque flavis. *Chectoo*, of the Javanese. A bird of social habits, resorting to the vicinity of human dwellings.

Brachypteryx. Char. essent.—*Rostrum* culmine inter nares carinato lateribus planis apicem versus rotundato lateribus convexis, tomiis subinflexis. *Alæ* brevissimæ, obtusæ. *Cauda* mediocris, rotundata. *Pedes* elongati, debiles; tarsi graciles, digiti gracillimi unguibus compressissimis hallucis majore.

B. montana. B. nigricante-cærulea nitore cano, subtus pallidior, abdomine albido, supra oculos maculâ albâ, dorso alis supra caudâ basi hypochondriis tibiisque (mari) badiis. *Ketek*, of the Javanese. A resident in the forests of Mount Prahu.

Motacilla speciosa. M. atra, pileo cristato ventre uropygio fascia alarum rectricibus extimis totis, ceteris apicibus niveis, caudâ longissimâ forficatâ.—*Chenginging*, or *Kingking*, of the Javanese. Found exclusively near small rivulets, and almost entirely confined to the southern coast of Java.

Tribus Conirostres. Fam. Corvidæ.

Phrenotrix. Char. gen.—*Rostrum* mediocre, validum,

* This is a curious musical fact, but seems to be at variance with the position of Mr. Blackwall, *supra* p. 22. note.—Edit.

altum,

altum, cultratum, basi crassiusculum. *Maxilla* arcuata, lateribus subconvexis, lævibus, sensim in culmen conniventibus. *Capistrum* latum, plumulis holosericis densis. *Nares* capistro reconditæ, circulares, parvæ, in medio sulci transversalis ad basin rostri dispositæ. *Alæ* rotundatæ: remigibus integris 3 et 4 longioribus. *Cauda* corpore longior, cuneata, rectricibus 10 in paribus dispositis. *Pedes* congrui: digiti mediocres, exteriore medio ad basin levissimè coalito. *Acropodia* scutulata. *Ungues* compressi: hallucis medio vix major.

P. Temia. P. fuliginosa nitore viride-olivaceo fuscescente, capistro atro.—*Chekitut*, or *Benteot*, of the Javanese: *Temia* of Le Vaillant. Only observed near solitary hamlets situated in tracts recently cleared for cultivation, where its food is abundantly supplied by the insects contained in the rich mould, and by the wild fruits about the skirts.

Tribus Scansores. Fam. Certhiadæ.

Prinia. Char. essent. — *Rostrum* apice validiusculum. *Maxilla* basi recta, apice levissimè arcuata. *Mandibula* myxa longiuscula, sursùm inclinata. *Tomia* maxillæ mandibulæque integerrima. *Alæ* breves obtusæ. *Cauda* elongata cuneata. *Tarsi* graciles longi.

P. familiaris. P. olivaceo-fusca, abdomine flavo, gulâ pectore fasciisque duabus alarum albis, rectricibus, intermediis subconcoloribus exceptis, fasciâ latâ subterminali perfuscâ. *Prinya*, of the Javanese. Abundant in many parts of Java, near villages and gardens.

Fam. Cuculidæ.

Cuculus lugubris. C. ater nitore viridi, remigibus exterioribus pogonio interno albo notatis, rectricibus duabus externis crissoque albo fasciatis, tibiis posticè albis.—*Awon-awon*, of the Javanese. Found in districts of secondary elevation, which are diversified with extended ranges of hills, and covered with luxuriant forests, in the southern and western parts of Java.

C. xanthorhynchus. C. violaceus, axillis ventre cruribus rectricibusque externis albo fasciatis, rostro flavo. Found in Java, but rarely.

Centropus Philippensis, var. *Javanica.* C. cyaneo-nigro nitens, alis badiis. *Bubut*, of the Javanese: *C. Philippensis* Cuvier.

"The Bubut," Dr. Horsfield observes, "affords a good illustration of the genus *Centropus*. Among the species which compose it, is a small group, the individuals of which have a general agreement both in their external covering and in their cry, although distributed through very distant countries. They are found not only in New Guinea and in the Philippine and Sunda Islands, but their range extends to Madagascar, and thence through

through the continent of Africa to Senegal and Egypt. The species which constitute this small group have as yet not been clearly defined; and by several ornithologists of the first eminence the adult and the young bird are described with different denominations. Although in the Systematic Description of Javanese Birds the Bubut has been enumerated as a distinct species, a re-examination of the various specimens in our collection, and a comparison of the varieties in size and external marks with the figures of Buffon and Le Vaillant, have induced me, at least for the present, to unite it with the *Centropus Philippensis* of Cuvier. It tends to confirm the near relation which exists between the species composing the small group above mentioned *; their note has suggested nearly the same name in the most distant countries: it is *Bubut* with the Javanese; *Houhou* in Egypt; and *Toulou* in Madagascar. They likewise resemble each other in their manners and their food. In the islands of the Indian Archipelago, as well as in Egypt, they are seldom seen in forests, but frequent low bushes; they live solitary, or in single pairs, and they feed chiefly on locusts. Their external covering is similar, both regarding the distribution of colours and the particular properties of the plumage. The feathers of the head, neck, back, breast, abdomen, and tail, have a very dark blue tint inclining to black, with a strong gloss, which is purple on the summit of the head and neck, yellowish-green with a metallic lustre on the tail, and more uniform and dark underneath. But it varies in different individuals according to their age, and according to the light to which they are exposed. A peculiar property of the plumes covering the head and neck, which belongs to all *Centropi*, shows itself more strongly in the group which comprises the Bubut. The plumes are very rough and rigid, and the barbs are separated and again subdivided, constituting, according to Illiger, decompound plumes with bristly lateral filaments. The tail is gradated, and consists of ten broad feathers, decreasing regularly in size from the intermediate ones to the two exterior feathers, which are abruptly shorter."

Phœnicophaus Javanicus. P. cano-viridescente niger, malis gulâ jugulo crisso cruribusque ferrugineo-badiis, rectricibus apice albis.—*Bubut-kembang*, of the Javanese. Found in the society of various species of *Centropus*, at the confines of large forests, in plains covered with low shrubs, and solitary trees.

* I consider this small group to consist of the following species, agreeably to M. Cuvier's arrangement, as given *Règne Anim.* 426, in the note :— 1, *Cuculus Ægyptius* and *Senegalensis*, which are united by M. Cuvier. 2, *Centropus Philippensis*, Cuv. 3, *Centropus nigrorufus*, Cuv. 4, *Centropus Tolu.*

Tribus

Tribus Tenuirostres. Fam. Cinnyridæ?

Pomatorhinus. Char. essent. *Rostrum* validiusculum, integrum, ultra nares abruptiùs compressum. *Nares* operculo corneo, convexo tectæ. *Alæ* rotundatæ. *Cauda* elongata. *Unguis* hallucis validior.

P. montanus. P. castaneus, capite cinerescente-nigro, strigâ oculari de rostri basi supra oculos ad nucham extensâ gulâ pectoreque albis.—*Bokkrek*, of the Javanese. It belongs to those tribes of birds which in Java are found exclusively in forests, covering mountains that have an elevation of about 7000 feet above the level of the ocean.

Ordo Rasores. Fam. Tetraonidæ.

Perdix personata. P. supra fusca subtus cinereo-fuscescens, gulâ collo anticè et ad latera lineâque superciliari albis, pileo colloque posticè et torque collari nigris, alis abdomine crissoque maculis transversis lunulatis nigris et castaneis.—Formerly *P. orientalis* of Dr. Horsfield. Inhabits the elevated woods of the province of Blambangan, near the eastern extremity of Java.

Ordo Grallatores. Fam. Ardeidæ.

Ardea speciosa. A. cristata alba, dorso nigro, collo supremo flavescente anticè pectoreque rufis, rostro basi albente.—*Blekko-irêng*, of the Javenese: *A. malaccensis*, and *A. senegalensis*, Linn. ed. Gmel. Very abundant in Java.

Fam. Scolopacidæ.

Scolopax saturata. S. saturato-fusco castaneoque varia, supra fasciis alternis inæqualibus, collo anticè pectoreque æqualibus, abdomine et crisso dilutioribus, occipite obscuris.—*Tekken*, of the Javanese. Inhabits the forests of Mount Prahu.

Fam. Rallidæ.

Parra superciliosa. P. atro-viridis nitens, lineis superciliaribus albis, dorso alisque viridi-olivaceo nitentibus, remigibus nigris, uropygio caudâque castaneo-rufis nitore violaceo.—*Pichisan*, of the Javanese. Found on the continent of India, and in the islands of the Indian Archipelago.

Ordo Natatores. Fam. Anatidæ.

Anas arcuata. A. supra nigrescente fusca subtus castanea abdomine saturatiore subvinaceo, alis supra medium badiis, collo lunulis nigris arcuato, plumis dorsalibus arcuatim fasciâ badiâ terminatis, capite supra nuchâque fuscis, gulâ pallidiore.—*Meliwis*, of the Javanese.

The

The eighth and concluding Number of this work, in addition to the descriptions given in it, contains a general catalogue of Javanese Birds, arranged in the museum of the East India Company. And now, having presented our readers with a full analysis of Dr. Horsfield's Zoological Researches, it only remains for us to express our wishes that we may soon have occasion to review some further results of his valuable labours.

XX. *Proceedings of Learned Societies.*

Prospectus of a School of Mines in Cornwall. By John Taylor, *Esq. F.R.S. Treas. Geol. Soc.* *

THE progress of improvement in the art of mining has been exceedingly rapid, in some parts of England, during the last twenty years, and it might be curious and interesting to endeavour to trace the causes of this advanced state of a science which embraces so many others, and which has to contend with so many natural difficulties. Much must be owing to a more general diffusion of knowledge, and to a better understanding both of practical details, and of the œconomy of their application; the growth of capital has afforded the means for exertion, increasing difficulties have stimulated that exertion, and intelligence and an improved acquaintance with natural science has made it effective.

Fifty years ago the mines of Cornwall were nearly at a stand, and no power existed by which they could be carried deeper and their richness further explored. The history of the steam-engine will show that mining encouraged the first inventors, or improvers, of this wonderful machine to pursue their labours, and that while the mines tended to produce what has so much benefited all classes of manufacture, they gained, in their turn, not only the direct power which this new auxiliary at once afforded them, but many ingenious men were associated with the managers of the deeper mines, whose speculations and inventions acted by a twofold influence to extend the means and to enlarge the intelligence of those who were occupied in enterprises of this kind.

Such, probably, are some of the causes which have given an impulse to the improvements in the art of mining; and I have, as I believe, during an acquaintance with the subject, and with some of the principal persons connected with mines, in the last twenty-six years, often remarked the workings of these in-

* This Prospectus has been circulated among those who are principally concerned in mining pursuits: we insert it, as we think the public cannot fail to take an interest in the success of the proposed establishment.

fluences. All this has, however, taken place in England without the help of two things, which it may be thought would be most conducive to the end, and which are possessed in other countries celebrated for their skill in mining:—first, the means of public instruction in the sciences upon which the miner founds all his processes; and, secondly, books in which the art of mining is treated, and the experience of one set of men is transferred to many.

We have nothing in the whole kingdom analogous to the schools of mines of Germany and Hungary, nor any institution where miners of higher or lower rank can learn their profession; and, with regard to books upon the subject, nothing can be more meagre than the whole collection of English works, which, indeed, are rather curiosities, and calculated to show what mining once was, than to teach what it ought to be.

The practical management of mines must always be deputed to those who have gained a most important part of their experience by actual work underground; and it would be as unreasonable to expect a landsman to rig and manage a ship, as to place the detail of extensive mines in the hands of those who have not encountered the casualties by which they are beset, and thus acquired the knowledge necessary to overcome or avoid them.

The education desirable for a miner is then a peculiar one, and must be adapted to go with the necessary labour, and not to supersede it: it should explain and make clear the reasons for each proceeding, not make the scholar unfit for his proper duty; it should not tend to the paths of theory and dispute, but show that good practices depend upon solid and intelligible principle.

I believe I may easily show that such an education is compatible, to a considerable extent, with the necessary state of industrious labour; and if I do this, I believe it may be sufficient to recommend the attempt,—for I hardly expect to have it objected that, as great progress has been made without it, instruction is unnecessary; nor can we now anticipate any of the exploded arguments against the diffusion of knowledge in general.

That the improvement has taken place without education cannot, indeed, be urged; for I have endeavoured to show that it has been the result of a more enlarged communication, which is itself education: and if the effect has been great from what has been imperfectly communicated, there is no difficulty, I conceive, in admitting that from a more regular and well digested plan of education much more good would be obtained.

It is contended, and as I believe most truly, that the present means for the acquisition of knowledge, which various admirable institutions afford to the most intelligent classes of artisans, are likely to produce a more favourable change in the state of society than any thing hitherto proposed with a view to enlarge the usefulness of a most valuable class of men: if this be so, there can be no reason why such advantages should be withheld from the miner; unless it can be shown that it will do his art no service, or that the persons who are to be taught are incapable or unworthy of such education.

On both these points I may first observe, that they seem to have been settled long ago by the example of the Germans and Hungarians, who have, until lately, been deemed, and perhaps justly, the masters of the art. Their colleges of Freyberg and Schemnitz have been long in existence, and have always been organized for, and devoted to, the purposes I recommend: if, therefore, what has become the theme of praise in other parts of Europe be not applicable to England, it must be either because our mines do not require intelligence and skill for their management, or that our miners are not likely to have their intelligence and skill advanced by the most obvious means for doing so.

As to the first, it is well known that one effect of late efforts in mining in England has been to deepen the mines with a rapidity totally unprecedented, to consolidate smaller concerns into larger ones, to explore more perfectly the ground in all directions, to adapt means that might render labour productive of profit, to stimulate the labourer by combining his interest with that of his employer, to watch every symptom with care, and to employ every device that ingenuity could suggest to overcome difficulties. It must then obviously follow that there is a greater demand for skill in the conduct of these affairs: as the mines are increasing in depth and extent, numerous expedients to counterbalance these difficulties are required; and as the expenses increase, compensations must be looked for in the aids that science may afford. Many mines, even in England, are yet shallow, and are drained by levels from the nearest valleys: here the operation is a simple one; and those who are accustomed to work them know but little of the means by which water is drawn from great depths, or ventilation is produced under certain circumstances which exist where works have been pushed down even far below the surface of the sea. All mines were originally in the state of those above alluded to; and no other good expedient being known, a limit was put to their extent and productiveness. Ingenuity and science have removed the barrier from those who are placed in the way of

instruction and comparison; but to many, who have not these advantages, the door of improvement yet remains in a manner closed: they hear indistinctly of the progress of others, but, not having the means of judging rightly, they are too apt to fence themselves in with prejudice and conceit, and to resist what has not originated with themselves. Nothing, in my mind, could do so much towards the removal of such narrow views, as a School of Mining, where young men, while they could continue their practice underground, might hear those sciences explained which would be most useful to them, and might devote some of the spare hours which a miner's life affords, to seeing and comparing the practice of others in a place where their art has reached the greatest perfection.

With regard to the capacity of the working class of miners for instruction, but little requires to be said: it is, I conceive, admitted by those who know them best, that they possess it in a peculiar degree. From this class the agents even of the largest mines are taken; and if I were to say much of what I think of the talents that they commonly possess, and of the excellent use they make of the means of instruction, slight as they are, which are thrown in their way, it might appear that I meant to flatter men with whom I am much associated and to whom I am so much indebted;—but the fact is notorious, in Cornwall particularly, that education is much sought after among the miners, and that its benefits are improved as much as the means will admit, and even frequently far beyond what could be expected from the few opportunities at present afforded them. Miners in general are a superior class of men, and, in the deep mines particularly, the constant exercise of judgement and thought which is necessary, produces a proportionate degree of intelligence.

In the army a great effect has been produced by the course of instruction provided of late years for the corps of Sappers and Miners; and under judicious arrangement even the privates have been taught so much of the science of their duties, as to have produced numerous instances of men well qualified to direct, without having rendered them less disposed to obey. Surely then it is highly probable that a similar effect might be produced upon men whose duties are not unlike, and are even more varied and difficult.

The demand for intelligent and well-instructed miners is now greater than at any former period; our deep mines are most extensively worked, from the increased call for the metals they produce, and the facility with which capital is obtained for such undertakings; the shallow mines are daily approaching to the state when more skill is required, and when machinery

machinery, before unnecessary, must be applied and depended upon. The mines of distant countries are passing into the hands of English possessors, and they must rely more or less for their success upon the talents of the agents they employ. A School of Mines will not only be the means of instructing such agents, but it will be a place where character will be developed, and as it were put upon record;—it will be a point to look to by those who require good agents, and it would in time become most valuable, as such, to all those who are either concerned in mines in the district where the establishment may be, in those of other parts of the United Kingdom, or to those numerous companies who are embarking their capital in distant parts of the world. I may, I think, without chance of disappointment, invite the attention of all who are thus engaged to the subject, and recommend it to their effective support.

That the mines should be skilfully and properly worked is no less the interest of the land-owner than of the adventurer; and I need hardly say more than this to recommend such an institution to their patronage, knowing, as I do, from many of the highest rank, that they are willing and anxious to afford it.

If I have undertaken the task of showing how I think such a plan may be carried into effect, it is not because I wish to dictate any particular scheme, or to originate a measure which I desire most earnestly to promote; but because I have been invited to do it by those whose judgement I most highly respect, and who have thought that from my connexion with their concerns I may be able to draw an outline which may serve to elucidate the design and promote its execution. I regret that the pressure of arduous business has delayed my exertions in this cause, and on many accounts I fear that my efforts will be very inadequate to the importance of the undertaking. I proceed, however, to detail my views of the mode in which such an institution may be established and conducted.

I propose to arrange the detail under the following heads:

1. The things most proper to be taught.
2. The class of persons who may be expected to be scholars.
3. The professors.
4. The situation of a School of Mines.
5. The probable expense of the institution.
6. The means of providing the necessary funds.
7. The government or direction.

1. *The*

1. *The things most proper to be taught.* — I might perhaps be expected to arrange the sciences which I shall mention in a different order from that in which they will appear; and I may be thought to lay more stress on some, and less on others, than some of my scientific friends may approve; but I beg it to be recollected that my wish, as I stated in the early part of the paper, is to prefer solid and intelligible principle to all subjects of mere theory and dispute; and I desire to give my opinion now as to what will, in my judgement as a practical man, be most eminently useful to those who are to assist in the management of mines. I may therefore insist less upon matters of curious research; but it is because I am convinced they are not so important as they are often stated to be.

1st Class. In this I would propose studies that may be called mathematical, which will be essential as the ground-work of many others and as highly important in themselves. The subjects principally attended to should be

Certain branches of Arithmetic—Geometry—Mensuration —Surveying, Dialling, and Levelling—Illustrative Drawing and Mapping.

2d Class. Natural Philosophy, selecting the following objects as those which I consider of the first importance:

Mechanics and practical illustrations of the application of power—Hydraulics and Hydrostatics—Pneumatics—Machinery and the Steam-Engine—Strength of materials—Carpentry and construction in general.

Other branches of the science may be taught, but they may be considered as subordinate.

3d Class. Geology—Mineralogy—Chemistry—Metallurgy —Assaying— History of Mining — Art of Mining in general.

Perhaps it may be difficult, at least for a time, to accomplish all this well; and, if that should be found to be the case, I should prefer the first two classes, as most required.

I do not wish to undervalue geology or mineralogy; but a practical knowledge of the latter at least, so far as it is really useful to the miner, is more generally diffused than is often supposed, and the demand for specimens for collections has doubtless contributed much to this. Some geological instruction would be highly desirable, and metallurgic chemistry would be important to some who might be intended for smelters, and to those also who though, owing to the division of labour in this country, they are usually not called on to interfere in the reduction of the ores, yet may sometimes have occasion

casion for the knowledge of the principles which ought to guide their practice.

The art of mining the pupils would be supposed to understand from their previous practical education and habits; but if a proper teacher could be found, to describe and point out the reasons for established practices, to direct attention to such matters as may call for improvement, and generally to sum up the application of the elementary studies to the art itself, it would be most desirable to have such an one. Probably a professor of this sort duly qualified could, at present, be found with difficulty, but the institution itself might very probably supply one at no very distant period.

2. *The class of persons* who I propose should receive benefit from such an institution would be principally those young men who, after having by actual labour qualified themselves to understand the practice, might be desirous of understanding what it is proposed to teach, and who might thus more perfectly qualify themselves for the important task of managing mines, or for becoming agents; to which duty a certain number are continually called. I would not, however, be understood to draw an exclusive distinction in this respect; but, while the school is intended principally for miners, I would certainly have it open to artisans who work in other branches, and particularly in those which are useful to mining.

It is probable that a class of pupils of somewhat higher rank might desire to avail themselves of instruction, fitted not only to such as may wish to understand mining, but to those who follow other occupations in which the sciences taught might be usefully applied.

Such students might be admitted, but under special provisions that the great object of the institution be not impeded or weakened in its effect.

I would propose that each miner and artisan should pay a small sum for each course of lectures, and be permitted to choose which he would attend.

Students of a higher class should pay at least so much as that they should be considered to be under no obligation to the funds of the institution.

As the periods of labour of the working miner are not very long, and so regulated as often to afford them considerable time during the day, the lectures might be regulated so as to be convenient for their attendance, without much interruption to their necessary duties.

3. *The Professors.*—Much of the success of the plan must doubtless depend on the choice of teachers; and though the branches of learning seem to be varied, yet I cannot help thinking

thinking that a great deal might be accomplished by a small number of professors duly qualified. I have arranged under one head, in the description of the three classes of instruction, those branches of science which are not unusually attended to by the same person; I think, therefore, that with three professors the institution would be tolerably perfect, and, if funds cannot be found for so many, I even think that two would produce a most beneficial effect.

4. *The proper situation* for a School of Mines must necessarily be where mines are most numerous and extensive, where the greatest variety and the largest quantity of mineral products are raised, where veins are most perfectly traced and explored, where the difficulties attendant on mining most frequently occur, and where they are overcome by the largest application of the power of machinery, and by the adoption of varied contrivance and skilful device. No part of England that I know of presents these requisites in an equal degree with that of the mining district of Gwennap and the surrounding country, Thomas's map of which may be referred to as confirming this opinion. The town of Redruth, nearly in the centre of this map, is, I think, beyond all doubt the place where such an institution should be fixed, as convenient for study and for the accommodation of those who might (as it may be expected some would do) come from a considerable distance.

5. *The Expenses* of such an institution should not be larger than prudence would dictate, with a proper view to the permanency of the establishment. Such limits ought to be assigned as would bring it within the probable means of support, even when mining may be in circumstances much more depressed than it now is; and they ought to be such as the miners, after having been practically convinced of the benefits to themselves, might by some effort reach to, in order to support it.

I expect that there will be no great difficulty in raising a sum by donations for the buildings and the necessary apparatus for proper lectures, and for a select practical library.

The erections should be of a plain description, and all attempts at ornament should be carefully avoided; nor is it necessary that they should be large. A lecture-room for about 200 students, apparatus room, chemical laboratory and assay-office, model-room, and a library are all that are essential. Three moderate dwelling-houses for the professors would be very desirable, because this would be in the nature of a permanent provision, as far as it went, towards their salaries; and in this view, and because houses are scarce in Redruth, I should wish to see them added to the erection. I have at present no accurate

accurate means of judging; but I think with due attention to œconomy, in a country where building materials are cheap, all this might be done for about 6000*l.*

I conceive that adequate professors might be had at salaries of 200*l.* per ann. each, allowing them houses and the right of making some other use of their time under proper restriction; it being also provided that the money subscribed by the students should be paid to the professors.

I would propose that these contributions by the working students should be very moderate, and the whole fixed by the governors: that each student should be allowed to subscribe to such lectures as he might prefer, and that the sum should be given to each particular professor exactly according to the number of his auditors, that it might influence each in rendering his instructions proper and efficient. I would not propose more than half-a-crown from each working student for each course of lectures. The students of other classes might pay much more, and the greater part of the sum collected from them might go to the general account of the institution. The courses of lectures should be long and in detail, and not more than two courses should be given on one subject in the year; and students entering for a whole year, or for the whole series, should pay at even a lower rate, so that the cost of the instruction should not exceed a very moderate annual charge.

Having made these observations, I will proceed to estimate the current expenses of the institution for each year.

	£.	s.	d.
Three professors	600	0	0
Materials for lectures, stationery, &c.	60	0	0
Purchase of books	40	0	0
Additions to apparatus and repairs of do.	100	0	0
Salary of librarian (one of the professors)	20	0	0
Porter and housekeeper	60	0	0
Repair of premises	50	0	0
Collector and secretary	100	0	0
	1030	0	0
Contingencies	70	0	0
	1100	0	0

6. *The means of providing the Funds.*—I cannot anticipate any serious difficulty in this respect, when I consider the large interests that, as it appears to me, will be benefited by such an institution; and I will venture to enumerate and classify those who may be expected to contribute. The order of arrangement I would propose would be that of benefit to be received, and the amount of contribution I should consequently expect they would offer.

I would therefore put at the head of the list the adventurers, or those persons who risk their capital in working mines. These may be divided into

The adventurers in the mines of Cornwall.
Persons working lead and other mines in other parts of the United Kingdom.
Companies engaged in foreign mines.
Iron masters and owners of collieries.

To all of these I conceive the advantages of such an institution must be more or less interesting; and if it be urged that, for the latter division especially, the peculiarities of their art will not be so much attended to, or that the position of the school is not the best, it may be answered that much of general interest to all classes of miners and smelters may be taught, and if adequate support be afforded, some extension of the plan might be made to suit particular views. Of course, also, I should expect a much smaller contribution than from those who are more nearly and decidedly interested in it.

With regard to miners in parts of the United Kingdom distant from Cornwall, I would suggest that many of them have taken their agents from the district where the school is proposed to be; and that, looking forward, they may either from its success derive a facility in procuring others, or may send their young men to be instructed at a place where the great advantage offers of seeing practices varied from what they have been used to.

The various associations that are sending miners to distant countries are, I think, specially called upon to support such an institution. Many of them feel the difficulty of procuring well recommended persons: it is but fair then that they should aid in training them up; and they will recollect that from such a school satisfactory accounts of character may always be obtained.

The adventurers of Cornwall and Devon are undoubtedly the most interested in the welfare of this undertaking, and it is from them that I expect the most effectual support. As one of this body, and representing a large number of them, I shall most cheerfully contribute, and urge it upon those with whom I have any influence.

The next class of contributors which I should be disposed to solicit for aid, would be the land-owners, or the lords of mines, who cannot be supposed to be indifferent to the skill and intelligence with which the works on their property are conducted. The rank of many is so high, and their means of judging so extensive, that it would be impertinent in me to do more than humbly to suggest this as a case for their liberal consideration;

consideration; and I wish not to be thought presumptuous when I do so to the principal officers of his Majesty's duchy of Cornwall, to the directors of the Royal Hospital at Greenwich, and generally to those distinguished noblemen and gentlemen whose names are connected with the mines of the empire.

The third class, from whom I should hope some help might be expected, would be that large number of enlightened individuals who are ready to promote any rational scheme for the extension of scientific knowledge, and particularly those who take part in the studies to which the art of mining contributes, or to which it is allied. If regular contributions cannot be relied upon to any great extent from such, or ought not perhaps to be expected, yet I may be permitted to remind them that in other ways their assistance may be most usefully extended to the institution; donations of books, of apparatus, of specimens in various departments, may be given, and in many instances important advice and information.

There is another class of persons, and a most numerous and important one, who perhaps might not think the school unworthy their support,—I mean the smelters of England, and those who are connected with the various processes of metallurgy for which the country is so celebrated. I have proposed that in the course of instruction the principles upon which the assay, the reduction, and the refining of metals depend should be taught, and the different processes elucidated; and it will only depend upon the means afforded how far this may be extended or how beneficially it may be employed.

I trust I might look to the proprietors of our great copper smelting works, to the lead and tin smelters, and to many others, for some permanent assistance.

I will proceed to show that from such extensive sources an income sufficient for the purpose may be derived without being burdensome to any; and I will first observe that I only calculate the necessary income, because I hope the sum which may be requisite to create the establishment would be readily given in the first instance in the way of donations.

The value of the soft metals now produced in these kingdoms is probably about as follows—

Copper	1,000,000
Lead	800,000
Tin	400,000
	£2,200,000

Iron, coal, and other products of the mineral kingdom I have no means of estimating, nor, if I had, should I be disposed to

infer that they should contribute in any proportion with those enumerated. If, however, the copper would contribute 500*l.* per ann. and the tin 150*l.*, the mines which produce these metals being near the spot where the institution is proposed to be fixed, and therefore called upon to do more than others, we might look for 200*l.* a year from the lead mines, and thus at once have a great part of what is absolutely required.

The remainder might fairly be left to the sources I have mentioned, and the hope indulged that a surplus income, for some years at least, would be raised to establish a permanent stock for future support.

The mode in which I would suggest this necessary revenue might be easily collected would be as follows:—

The amount of 500*l.* per ann. from the copper mines is, as near as may be, at the rate of one penny per ton of ore; and as this is nearly all sold at the ticketings in Cornwall and Swansea, I would propose it should be always added to the bill of the day's expenses, and received weekly.	£500	0	0
Copper mines which sell by private contract would pay the same amount upon the quantity of their ores.			
There are probably 6000 tons of tin ore sold to the smelters; and as each ton is worth seven or eight times the value of a ton of copper ore, the ratio might be taken at 6*d.* per ton, which the miners would probably not object to leave in the hands of the smelters when they sold their tin . .	150	0	0
The lead mines are so distinctly situated, and may be considered so variously interested in this affair, that I do not presume to offer any plan by which the persons engaged in them can be expected to levy a revenue with the same regularity or facility; but I should hope, from what I know of the wishes of many, that I might estimate their contributions at least at	200	0	0
Companies engaged in mines in Mexico, Columbia, Chili, Peru, the Brazils, &c., estimating a subscription from ten of the most considerable of 20*l.* each per ann.	200	0	0
Gentlemen engaged in carrying on iron works and working collieries	50	0	0
Many of the lords of mines in Cornwall, if the contributions which I have proposed be made in			
	£1100	0	0

ores,

Brought forward	£1100	0	0
ores, will have paid their proportion of that sum; and much more ought not, perhaps, to be expected from them. There are many, however, who would be inclined to patronize the institution further; and looking to the great means in the hands of those who possess mines on their estates, in various parts of the kingdom, I am inclined to hope for voluntary subscriptions for	250	0	0
The voluntary aid which would, I have no doubt, be offered by many friends of science who have no other interest in this plan than that which they constantly exhibit where the promotion of science is the object, may doubtless be reckoned upon to some extent, say	100	0	0
The copper companies, lead and tin smelters, and other respectable persons who may be concerned in arts which may receive improvement from the instruction proposed to be given, may amount to	200	0	0
Total annual income . . .	£1650	0	0

This statement shows a probable excess of revenue over the estimated wants of the establishment: there is no reason, however, to let this be a cause for diminished exertion in raising the money; after a short time, with more extended means, a larger plan of usefulness might be digested, but the great object, at first, would doubtless be to secure the permanency of the institution by a prudent investment of surplus income to create a fund which might become a settled source of revenue.

To encourage the subscriptions of distant land-owners or adventurers in mines, certain advantages in the nomination of students, or in facilities to be given to such as they might recommend, may be hereafter proposed and adopted.

7. *The government or direction.* On this subject it appears to me that the effective management must be vested in the hands of a select committee, who may have permanent or temporary residences near the spot where the institution is to be placed. The number need not be large, and there are many gentlemen connected with the mines who are well qualified for the duties they would have to perform.

There may be also another body of governors, to whom reports might be annually made, and who might assist the committee of management in the choice of professors and other important matters.

I would

I would propose that the following official gentlemen should always be governors:—

The Lord Warden of the Stannaries.
The Vice Wardens of Devon and Cornwall.
The Surveyor General of the Duchy of Cornwall.
The Great Barmaster of the King's Field in Derbyshire.
The President of the Royal Society.
The President of the Geological Society.
The President of the Royal Cornwall Geological Society.
The Professors of Geology and Mineralogy of Oxford and Cambridge.
The Secretary of Greenwich Hospital.
Ten Gentlemen to be named by the ten principal Copper Mines in Cornwall.
Four Gentlemen by the four principal Tin Mines.
Two Gentlemen by the Copper Companies.

And that each mine, out of Cornwall or Devon, which should contribute 10*l.* a year and upwards, should name a governor; and each individual whose personal subscription should be 5*l.* a year should be a governor.

The committee of management should probably continue in office for three years, a proportion of the number going out by rotation annually, their successors being elected by the governors.

The meetings of the committee of management to be held at the institution, and those of the governors might be held once a year in London.

All this, however, is respectfully submitted for discussion, not doubting but that many improvements and corrections of this imperfect sketch may be supplied by the intelligence and experience of my mining friends.

London, Feb. 7, 1825. JOHN TAYLOR.

P. S. If this proposal should meet with the approbation I am encouraged to hope for, I would suggest that it should first be considered by a meeting of the adventurers in the mines of Cornwall, who would determine whether they would afford to it that kind of support which I have ventured to propose.

ROYAL ACADEMY OF SCIENCES OF PARIS.

May 2.—M. Bogros read a memoir on the structure of the nerves.—M. Moreau de Jonnès commenced the reading of a memoir, entitled, "Monographic researches on the indigenous dog of the American hemisphere; the different species, their synonymy, forms, habits, domestic uses, extinction, geographical distribution, and migrations: and the notions to which

which they lead, respecting the ancient state of the New World, the communications of its inhabitants with each other, and their original country."

May 9.—The Minister of the Interior announced the arrival of M. Pacho at Derne, on the confines of Cyrenaïca.—M. de Humboldt presented some grains consisting of platinum, osmium, and iridium, found in the auriferous sands of the Ural. He also announced that he had received from MM. Boussingault and Riveiro an entire year of observations on the horary variations of the barometer, at Santa-fé-de-Bogotá. They have found that the mean monthly heights form a very regular series, and they have examined the influence of the moon on this barometer under the tropics.—M. Ch. Gaudichaud's memoir, entitled "Observations on the *Cycas circinalis*," was referred to MM. de la Billardière and du Petit Thouars.—M. Geoffroy St. Hilaire made a verbal report on the zoological portion of M. de Freycinet's voyage, drawn up by MM. Quoy and Gaymard.—M. Francœur read a memoir on the measures adopted in England for establishing uniformity of weights and measures.—M. Gondret read a memoir on cataract.

May 16.—M. Marcel de Serres transmitted a memoir on some remains of the mastodon *à dents étroites*, or mastodon of Simorre, recently found in several parts of France, and especially in the neighbourhood of Montpellier.—M. Mirbel, in the name of a committee, made a report on M. Gaudichaud's Flora of the Malouines.—M. de Freycinet read a memoir on the observations on the pendulum, made during his voyage round the world.—M. Moreau de Jonnès continued the reading of his memoir on the American dog.

May 23.—M. Jomard presented a map of the fall of the Nile, compared with that of several other rivers. He also communicated a letter from M. de Beaufort, dated from Basrel, respecting the route he is pursuing.—A new solar system by M. J. Telard was committed to the examination of MM. Mathieu and Cauchy.—M. Coquebert de Montbret, in the name of a committee of statistics, made a report respecting the prize in statistics, and proposed to award to the "Statistics of the department of L'Herault," by M. Hippolyte de Lessert, son of the prefect.—M. Geoffroy St. Hilaire, on the part of the committee for the prize in medicine, proposed the following: "Give the general and comparative history of the circulation of the blood, in the four classes of vertebrated animals, before and after birth, and at different ages."—M. Navier, in the name of a committee, announced that M. de Montyon's prize in mechanics had been adjudged to M. Pon-

M. Poncelet, captain of engineers, for his vertical water-wheels with curved float-boards, principally applicable to slight falls of water.—MM. Ampère and Dulong made a report on a memoir by M. Zamboni, relative to a dry galvanic pile.—M. G. St. Hilaire commenced reading a memoir on the general views respecting monstrosity, with the description of a new kind observed in the human species, named *aspalasome*.—M. Durville read a note on the observations and collections made during the last voyage round the world.

May 30.—A letter was read from Governor Sir T. Brisbane, communicating a series of astronomical observations made at Paramatta, by MM. Rumker and Dunlop; and the results of experiments made with the invariable pendulum.—MM. Duméril and Thenard made reports, in the names of the respective committees of medicine and public health, concerning the rewards adjudged under the will of M. de Montyon.—M. de Humboldt communicated to the Academy some specimens of the seleniurets discovered by M. Finke in the veins of the eastern part of the Hartz, and which M. H. Rose, of Berlin, has lately analysed. These minerals are compounds of selenium with lead, cobalt, mercury, and the air. M. Rose has also made some new researches on the combinations of antimony with chlorine and sulphur; on the analysis of iserine, rutile, and the titaniferous iron-ores; and on the best method of separating titanic acid and oxide of iron.—M. Pouillet read a memoir on the electricity of the gases, and on the causes of that of the atmosphere.

XXI. *Intelligence and Miscellaneous Articles.*

ADDITIONAL PARTICULARS OF THE BITBURG METEORIC IRON.

WE extract the following additional particulars of the Bitburg meteoric iron, described in our number for June, from a letter from Dr. Chladni to Professor Nœggerath, dated Magdeburg, Jan. 9, 1825; and given in Schweigger's Journal, Band xiii. p. 116.

"I thank you heartily for the account of the Bitburg mass of iron so kindly communicated to me, as well as for the piece of that celestial production, reduced by the barbarous smelting process to a common article, no longer fit for any other process. It was a very pleasant new-year's gift. But, although the original properties of this iron have been lost in smelting, if any one well knows the texture of compact meteoric iron, presenting itself in damasked figures as well as in the fracture, he

he may still find some traces of conformity with other compact meteoric iron.

"The surface shows distinctly that it consisted of small fragments of lighter and darker colours, which have been joined together by an imperfect process of smelting; and in some may be seen the remains of its former octahedral form and foliated texture. I tried a piece of about $1\frac{1}{2}$ inch square, by laborious filing and the application of aqua-fortis, for the purpose of discovering traces of the damasked crystalline figures; but I could not find any, and indeed could scarcely expect any after the process it had undergone. However, this surface also shows that the iron consists of small fragments joined together; and some parallel small fissures show that the principal surfaces of separation of the octahedrons were diagonal to the greater surfaces of the specimen *. If I did not know, from the account of Colonel Gibbs, and the preliminary analysis of Prof. Bischof, that this iron contains nickel, I should conclude so from its colour on the surface, which I polished and treated with the acid. The circumstance of this colour being of a darker gray than in other meteoric irons (that of the Cape of Good Hope excepted), is probably occasioned by the mixture of carbon in the smelting."

BROWN HÆMATITIC IRON-ORE FORMED AROUND CAST-IRON PIPES.

On examining a set of cast-iron pipes which had lain some years in the line of one of the streets in the New Town of Edinburgh, we were surprised to find the sand in which they had been laid, where in contact with the pipes, very compact and brown in colour. On breaking some of the masses, we found the connecting matter to be brown iron-ore, and in cavities of the compacted sand this brown iron-ore exhibiting that particular lustre approaching to adamantine, and the reniform shape with the granulated surface of brown hæmatite. Here, then, we have an instance of the formation, by the action of percolating water on the iron of the pipes, of an ore of iron which some observers arrange with the igneous mineral formations.—*Edin. Phil. Journ.*

THE RE-DISCOVERY OF THE COMET OF ENCKE DUE TO MR. RUMKER, AND NOT TO MR. DUNLOP.

In a paper published in the Transactions of the Royal Society of Edinburgh, (vol. x. p. 112, 113,) by Sir Thomas

* I should be inclined to consider these parallel fissures rather as oblong air-bubbles produced in the smelting, than as the remains of a former texture. —*Nœggerath.*

Brisbane, the merit of re-discovering the remarkable comet of Encke has been ascribed to Mr. Dunlop. On the authority of that paper, and of a private letter from Sir Thomas Brisbane, we afterwards contradicted a statement of Baron Von Zach, who attributed the discovery to Mr. Rumker. We have received, however, recent letters both from Sir Thomas and Mr. Dunlop, in which all the merit of the discovery is attributed to Mr. Rumker. Two comets had made their appearance at the same period in New South Wales, one of which was discovered by Mr. Dunlop, and the other by Mr. Rumker. It was, therefore, a natural mistake to attribute the discovery of the comet of Encke to Mr. Dunlop, and that of the other to Mr. Rumker, when it was exactly the reverse; the other comet of September 1822 having been discovered by Mr. Dunlop. We regret to learn that the health of that able and active astronomer Mr. Rumker has been so much impaired as to deprive the observatory of Paramatta of his valuable services. —*Edin. Journ. of Science.*

NEW COMET.

M. Biela discovered, on the 19th of July, a new comet in the constellation *Taurus*. It was seen about three weeks afterwards by Dr. Olbers. The apparent place of the comet was as follows:

		Æ	Dec. N.
July	19 =	61° 44′	26° 6′
	21 =	61 59	25 51
	23 =	62 12	25 49
	27 =	62 40	25 25
Aug.	3 =	63 18	24 43
	4 =	63 22	24 41
	9 =	63 40	23 54

PASTORFF ON THE SOLAR SPOTS AND CLOUDS.

In examining the sun's disk with a fine six-feet achromatic telescope of Fraunhofer, with powers varying from 25 to 400, M. Pastorff of Buchholtz, near Frankfort on the Oder, has observed several interesting phænomena relative to the spots on its surface, their penumbræ, and the phosphoric clouds. He observes, that the penumbræ of the spots resemble a mass of the empty eggs of the *Bombix neustria*, which surround the black spots concentrically and with different breadths. These apparent eggs are contiguous and, as it were, agglomerated the one to the other, with openings extremely small. M. Pastorff considers it quite certain that these spots with the penumbræ are on the surface of the solar globe, and that they disappear when the phosphoric clouds cover them, principally when they

they are near the margin of the sun; and he thinks that it is probably these phosphoric clouds which, in the interval of some hours only, form this great variety of spots. On the 1st December 1823, M. Brioschi of Naples observed a large spot equal to $1\frac{1}{2}$ our globe, surrounded with an irregular and branching elevation, into which there seemed to be precipitating great masses of fire. The whole surface of the sun he saw like an ocean on fire agitated by a storm. M. Pastorff saw this same spot on the same day, when the phosphoric clouds were in great motion; but though he has often seen the agitation of the phosphoric clouds much greater, he did not consider it as resembling an ocean on fire. Almost always, when the spots approach the margin of the sun's disk, they divide themselves into several groups, or they re-unite if they have been previously subdivided. Very near the margin, the spots appear totally altered, and they almost always appear as if they were dissolved and changed into luminous clouds, though that dissolution is only apparent; for it is quite evident that, in proportion as these spots approach the margin of the disk, the penumbra or the nebulosity which encircles them, covers them more and more till they totally disappear. There is then only seen the luminous nebulosity, which is sometimes surrounded with phosphoric clouds. The sun always appeared more bright at its centre than towards its edge.—*Edin. Journ. of Science.*

DR. GRANT ON THE EXISTENCE OF THE PANCREAS IN SOME SPECIES OF THE CUTTLE-FISH TRIBE, AND IN THE DORIS ARGO.

Dr. Grant lately read a paper before the Wernerian Society on certain glandular organs of the *Loligo sagittata* Lam., the most common species of *Calmar* of the Frith of Forth. These glands are situate at the lower and fore part of the liver, are two in number, consist of numerous distinct lobes, of a rose-red colour, and were formerly considered as the ovarium of this animal. It appears, however, that they surround the two biliary canals during their whole course from the liver to the spiral stomach, and communicate freely with the interior of these canals by numerous small ducts. They are always present, and equally developed, in the male and female, and have no organic connexion with the organs of generation. Coloured size injection, thrown into the digestive canal, passes up from the spiral stomach, through the two biliary ducts, and fills these glandular lobes in its passage. From the connexion of these glands with the biliary system, Dr. Grant considers them as analogous to the conglomerate pancreas of the skate, and other chondropterygious fishes, and is thus inclined to believe that this important digestive organ occurs lower in the

scale of animals than is generally supposed. Dr. Grant illustrated his observations by numerous specimens of the male and female, showing the viscera in their natural as well as injected state.

Dr. G. has also made some interesting observations on the nature of the glandular vermiform appendix opening into the stomach of several gasteropodous mollusca, as the *Aplysia*, the *Doris*, &c. From the relations of this small glandular cæcum to the biliary system and alimentary canal of these animals, and from its particular structure, Dr. Grant considers it as quite analogous to the small pyloric cæca, or proper pancreas, of osseous fishes, though representing that organ under a much simpler form. Several specimens of the *Doris Argo* were lately exhibited to the Wernerian Society, showing the connexions of this pancreatic appendix with the stomach and liver.—*Edin. Phil. Journ.*

PROFESSOR HOOKER ON MR. RUDGE'S FIGURE OF TRICHOMANES ELEGANS.

We copy from the Edinburgh Journal of Science the following statement occasioned by Mr. Salisbury's reference to Professor Hooker's plate of *Trichomanes elegans* mentioned in our report of the proceedings of the Linnæan Society, March 15, and April 5. Vol. lxv. p. 295.

"We find by an article in Taylor's Philosophical Magazine, that M. Bory de St. Vincent has declared the figure published by Mr. Rudge, in his *Icones et Descriptiones Plantarum Rariorum Guianæ*, of the *Trichomanes elegans* to be incorrect, and composed of two different species; or, according to M. Bory's ideas, of two distinct genera. This has given rise to considerable discussion among the botanists in London; and, in justification of the fidelity of the figure, our testimony is brought forward; we having given, in the fifty-second plate of *Exotic Flora*, a figure of the *Trichomanes elegans*, and having spoken of the figure of Mr. Rudge as excellent. This term of approbation, however, was only meant to apply to such [part] of the figure as represented that state of the plant which we had ourselves represented; that is, the barren fronds and those fertile spikes which have *separated* involucres. The other spikes with *united* involucres we had never seen; but having *then** only a single specimen to examine, we did suppose that

* "We say *then*, because we have since had the opportunity, through the liberality of the same gentleman as sent us the first individual (the Rev. Lansdown Guilding), of examining very many other specimens. All have the involucres *separated*, as represented in our plate, and as represented in the left hand spike of the entire plant in Mr. Rudge's representation, and at fig. 2. of the magnified portion."

those

those spikes which have the involucres united by a membrane might belong to a younger state of the fructification. On the specimen, however, from which Mr. Rudge's figure was made, (and which was gathered in Guiana by M. Martin,) being submitted to a careful examination, it was found to be composed of two individuals; thus, as it were, tending greatly to strengthen the opinion of M. Bory.

"It is, however, not a little remarkable, that Kaulfuss, in his work on the Ferns, who appears, from his manner of describing it, to be well acquainted with this plant, not only quotes the figure of Rudge, without questioning the correctness of it, but absolutely describes the *two* kinds of fructification represented by Rudge: first, in his specific character, "Indusiis spicatis distichè connatis, tandem liberis pedicellatis;" and afterwards in the description, "Indusia disticha, coarctata, primùm membrâna pellucidâ connexa, tandem distincta pedicellata spicam densam disticham subsecundam referentia." As a further evidence of his being well acquainted with the *Trichomanes elegans*, he corrects Willdenow, who, he says, only knew the plant from Rudge's figure, and who particularly described the fertile fronds otherwise than he would have done had he described from the plant itself."—(*H.*)

THE MENAI BRIDGE NEAR BANGOR, CARNARVONSHIRE.

On Tuesday, the 26th of April 1825, the *first* chain of this stupendous work was thrown over the Straits of Menai, in presence of an immense concourse of persons of all ranks. At half-past-two o'clock, about half flood-tide, the raft prepared for the occasion, stationed on the Carnarvonshire side, which supported the chain intended to be drawn over, began to move gradually from its moorings, towed by four boats, with the assistance of the tide, to the centre of the strait, between the two grand piers: when the raft was properly adjusted, and brought to its ultimate situation, it was made fast to several buoys, anchored in the channel for that specific purpose. The whole of this arduous process was accomplished in twenty-five minutes. The end of the chain, pending from the apex of the suspending pier on the Carnarvonshire side down nearly to high-water mark, was then made fast by bolts to that part of the chain lying on the raft; which operation was completed in ten minutes. The next process was fastening the other extremity of the chain (on the raft) to two immense powerful blocks, for the purpose of hoisting the *entire* line of chain to its intended station, the apex of the suspending pier on the Anglesea side. When the blocks were made secure to the chain (comprising twenty-five ton weight of iron), two capstans,

and

and also two preventive capstans, commenced working, each propelled by twenty-four men. To preserve an equanimity [equability ?] in the rotatory evolutions of the two principal capstans, a fifer played several enlivening tunes, to keep the men regular in their steps; for which purpose they had been previously trained. The chain rose majestically, and the gratifying sight was enthusiastically enjoyed by each individual present. At fifty minutes after four o'clock, the *final* bolt was fixed, which completed the whole line of chain. From the casting off of the raft, to the uniting of the chain, took up only two hours and twenty minutes.

This splendid specimen of British architecture will be a lasting monument to the discernment of the present government, for having called into requisition the transcendent talents of Mr. Telford, who was present on the occasion.

Upon the completion of the chain, three of the workmen had the temerity to pass along the upper surface of the chain, which forms an inverted curvature of 580 feet. The versed sine of the arch is 43 feet.

The following is a summary account of the dimensions of the bridge:—The extreme length of the chain, from the fastenings in the rocks, is about 1600 feet. The height of the road-way from high-water line is 100 feet. Each of the seven small piers, from high-water line to the spring of the arches, is 65 feet. The span of each arch is 52 feet. Each of the two suspending piers is 52 feet above the road. The road on the bridge consists of two carriage-ways, of 12 feet each, with a footpath, of 4 feet, in the centre. The carriage-roads pass through two arches, in the suspending piers, of the width of 9 feet, by 15 feet in height to the spring of the arches. To counteract the contraction and expansion of the iron, from the effect of the change of the temperature in winter and summer, a set of rollers are placed under cast-iron saddles, on the top of the suspending piers, where the chains rest. The vertical rods, an inch square, suspended from the chains, support the slippers for the flooring of the road-way, the rods being placed 5 feet from each other. The chains, 16 in number, contain 5 bars each: length of the bar 9 feet 9 inches; width 3 inches by 1 inch square, with 6 connecting lengths at each joint 1 foot 6 inches by 10 inches, and 1 inch square, secured by two bolts at each joint, each bolt weighing about fifty-six pounds. The total number of bars, in the cross section of the chains, is eighty.

A second chain was drawn over on Thursday morning, the 28th; and there are fourteen other chains in readiness to be drawn over when the tide will serve, which will complete the line of suspension.—*Edin. Journ. of Science.*

LIST OF NEW PATENTS.

To Peter Williams, of Leeds, and James Ogle, of Holbeck, Yorkshire, cloth-manufacturers, for improvements in fulling-mills or machinery for fulling and washing woollen cloths or other fabrics.—Dated 20th July 1825. —6 months to enrol specification.

To Charles Friend, of Bell-lane, Spitalfields, for improvements in the process of refining sugar.—26th July.—6 months.

To John Reedhead, of Heworth, Durham, for improvements in machinery for propelling vessels in marine and inland navigation.—26th July. —2 months.

To Edward Brooke and James Hardgrave, of Leeds, woollen manufacturers, for improvements in, or additions to, machinery used in scribbling and carding wool, &c.—26th July.—6 months.

To David Oliver Richardson, kerseymere and cloth printer, and William Hirst, manufacturer, both of Leeds, for improvements in printing or dyeing woollen and other fabrics.—26th July.—6 months.

To James Kay, of Preston, for his improved machinery for preparing and spinning flax, hemp, &c.—26th July.—6 months.

To Richard Witty, of Sculcotes, Yorkshire, for an improved chimney for Argand and other burners.—30th July.—6 months.

To Joel Lean, of Bristol, for a machine for effecting an alternating motion between bodies revolving about a common centre or axis of motion, also certain apparatus for applying the same to mechanical purposes.— 30th July.—6 months.

To the Rev. William Barclay, of Auldeare, in the county of Nairn, for an instrument to determine angles of altitude or elevation without the necessity of a view of the horizon.—30th July.—2 months.

To Richard Badnall junior, of Leek, for improvements in the manufacture of silk.—30th July.—6 months.

To Marc Lariviere, of No. 21, Frith-street, Soho, (late of Geneva,) for a machine for perforating metal plates of gold, silver, tin, platina, brass, or copper, being applicable to all the purposes of sieves hitherto employing either canvass, linen, or wire.—5th Aug.—6 months.

To Samuel Bagshaw, of Newcastle-under-Line, for his new method of manufacturing pipes for the conveyance of water, &c.—8th Aug.—2 mon.

To George Charleton, of Maidenhead-court, Wapping, and William Walker, of New Grove, Mile End Road, for improvements in the building of ships.—10th Aug.—6 months.

To Samuel Lord, James Robinson, and John Forster, of Leeds, for improvements in machinery for raising the pile on woollen cloths, and in pressing the same.—11th Aug.—2 months.

To William Hirst, Henry Hirst, and William Heycock, and Samuel Wilkinson, of Leeds, for their apparatus for preventing carriages from overturning.—11th Aug.—6 months.

To John Stephen Langton, of Langton-juxta-Partney, Lincolnshire, esq. for an improved method of seasoning timber and other wood.—11th Aug. —6 months.

To Jacob Perkins, of Fleet-street, for certain improvements in the construction of bedsteads, sofas, and other similar articles: communicated from abroad.—11th Aug.—6 months.

To Henry Richardson Fanshaw, of Addle-street, London, for his apparatus for spinning, doubling, and twisting or throwing silk.—12th Aug.— 6 months.

To Philip Taylor, of the City Road, Middlesex, engineer, for certain improvements in making iron.—18th Aug.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. CARY in London, and Mr. VEALL at Boston.*

	Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M.							Clouds.							Height of Barometer, in Inches, &c.		Thermometer. London.			Thermometer. Boston.	Rain.		Weather.		
		Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	8½ A.M.	London.	Boston.	London.	Boston.	Wind.
July	26	30.28	61	52·20	56	N.	0·40	.....	...	...	1	...	1	1	...	30·31	29·87	60	66	52	61	...	...	Fair	Cloudy	NW.
	27	30·24	63	.....	54	NE.	·40	.....	1	...	1	...	1	...	...	30·26	29·90	60	72	56	62	...	...	Fair	Fine	W.
	28	30·17	65	.....	52	NE.	·37	.....	...	...	...	...	...	...	...	30·20	29·80	61	73	57	62·5	...	...	Fair	Fine	SW.
○	29	30·15	63	.....	56	NE.	·33	.....	...	...	1	1	...	...	...	30·18	29·80	62	73	56	62	...	...	Fair	Fine	SW.
	30	30·11	64	.....	56	E.	·40	.....	1	...	1	...	...	...	...	30·18	29·74	62	71	61	63	...	...	Fair	Fine, 3 *p.m.* 76.	SE.
	31	30·00	69	.....	49	E.	·40	.....	1	...	1	...	...	...	...	29·99	29·50	64	76	66	67	...	...	Fair	Fine	SW.
Aug.	1	30·03	72	52·70	48	E.	·50	.....	1	1	1	...	1	1	...	30·02	29·50	66	82	73	71·5	...	...	Fair	Fine [with thunder	NE.
	2	30·00	72	.....	59	SW.	·15	0·040	...	...	...	...	1	1	1	30·01	29·40	70	69	66	66	...	...	Showery	Cloudy, rain *p.m.*	E.
	3	30·00	67	.....	58	SW.	·12	·105	1	1	1	...	1	1	1	29·97	29·43	63	70	64	65	...	·15	Fair	Cloudy	S.
	4	29·63	68	.....	64	SW.	·10	·820	1	...	1	...	1	1	1	29·57	29·05	66	68	60	63	...	...	Showery	Cloudy, rain *p.m.*	S.
	5	29·54	64	.....	58	W.	...	·035	1	1	1	...	1	1	1	29·60	28·83	60	70	60	60	...	·85	Fair	Cloudy, rain *p.m.*	S.
	6	29·67	65	.....	61	SW.	...	·100	1	1	1	...	1	1	1	29·65	29·10	61	65	59	60·5	0·55	·05	Showery	Fine, rain *p.m.*	S.
☾	7	29·78	66	53·00	60	SW.	...	·140	...	...	1	...	1	1	1	29·82	29·00	60	72	60	63	...	·09	Fair	Fine, [thunder	SW.
	8	29·72	66	.....	65	SW.	·78	·045	1	...	1	...	1	1	1	29·70	29·20	61	71	58	62	...	...	Showery	Cloudy, rain with	SE.
	9	29·77	62	.....	57	W.	...	·085	1	1	...	...	1	1	1	29·74	29·24	60	68	55	62	...	·13	Fair	Cloudy	SW.
	10	29·73	63	.....	59	NW.	...	·190	1	1	1	...	1	1	1	29·77	29·30	57	64	54	61	...	...	Showery	Fine, rain *p.m.*	S.
	11	29·98	60	.....	61	NW.	...	.....	...	...	1	...	1	1	...	30·04	29·55	55	66	60	58	...	·12	Fair	Rain	W.
	12	30·08	63	.....	62	NW.	...	.....	1	...	1	...	1	1	...	30·01	29·62	60	67	60	61	...	...	Fair	Fine	S.
	13	29·70	64	53·05	75	SW.	·65	3·65	1	1	1	...	...	1	1	29·50	29·13	60	63	59	61	0·70	·20	Rain	Rain and [Stormy	S.
●	14	29·50	62	.....	61	W.	...	0·25	1	1	1	...	1	1	1	29·53	28·90	58	65	55	62	...	·18	Fair	Stormy	SW.
	15	29·52	62	.....	62	NW.	...	.....	1	1	1	...	1	1	...	29·60	29·05	56	62	54	57·5	...	...	Cloudy	Rain	NW.
	16	29·84	65	.....	58	NW.	·35	.....	1	1	1	...	1	1	...	29·89	29·40	55	67	58	59·5	...	·08	Fair	Cloudy	W.
	17	29·89	63	.....	64	NW.	...	0·40	...	1	1	...	1	1	1	29·91	29·47	60	71	63	58	...	...	Fair	Fine, rain at night	SW.
	18	30·00	62	.....	68	N.	...	.....	...	...	1	...	1	1	...	30·10	29·60	59	66	58	60·5	...	·03	Fair	Cloudy	NW.
	19	30·20	60	53·50	64	N.	·55	0·40	...	1	1	...	1	1	1	30·20	29·75	57	60	54	57·5	...	...	Fair	Cloudy	SW.
☽	20	30·34	58	.....	75	NE.	...	.....	...	...	...	...	1	...	...	30·38	29·95	54	73	65	58	0·05	...	Fair	Fine	SW.
	21	30·34	69	.....	62	NW.	...	.....	...	...	1	...	...	1	...	30·36	29·85	60	70	62	67	...	...	Fair	Cloudy	W.
	22	30·28	64	.....	64	NE.	·60	.....	...	...	1	...	...	...	...	30·31	29·85	60	71	60	65	...	...	Fair	Fine	SE.
	23	30·15	65	.....	70	NE.	...	.....	...	...	1	...	1	...	...	30·17	29·76	60	72	61	61·5	...	...	Fair	Cloudy	E.
	24	30·10	63	.....	59	NE.	...	.....	1	...	1	...	1	...	...	30·14	29·68	55	71	59	66·5	...	...	Fair	Cloudy	E.
	25	30·11	63	53·85	72	E.	·70	.....	1	...	1	...	1	...	...	30·16	29·73	57	70	58	65·5	...	...	Fair	Fine	SE.
Aver. :		29·963	64·29	53·05	60·9		6·80	2·030	18	12	27	1	24	21	13	29·98	29·48	60	75	59	62·2	1·30	1·89			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

30th *SEPTEMBER* 1825.

XXII. *Experiments on Anthracite, Plumbago, &c.* By Lardner Vanuxem.*

THESE experiments were undertaken with a view to determine whether the globules obtained by Professor Silliman, from the above substances, were owing to a fusion of their carbon, or merely to the impurities or foreign matter contained within them†. They were long delayed by my waiting for some sheet zinc necessary to repair a deflagrator intended to be used for the purpose of obtaining the globules; but this not arriving, I resolved to avail myself of the suggestion of Professor Silliman; namely, that of using the compound blowpipe, which answered perfectly well. In the experiments with the blowpipe, the substances were placed upon platina foil, spread upon a lump of magnesia; the size of the pieces subjected to its action was about half an inch in diameter and a quarter of an inch in thickness. The light in the greater number of instances was so intense, that I found it necessary to use double green glasses.

The mode pursued in the analysis of anthracite and plumbago was as follows. The presence of water was ascertained by heating a few small pieces of the substance in a glass tube closed at one end; and the quantity of water, by heating a given portion in a covered platina crucible. Another portion was pulverized in an agate mortar; then a given weight of it was put into a platina crucible, and kept without its cover at a red heat in a small French furnace until the whole of the carbon was consumed; the residue was then boiled in water for an alkali; after which operation it was heated with caustic potash in a silver crucible: when the fusion of the mass was completed, water was added, and the whole then dissolved

* From the Journal of the Academy of Natural Sciences of Philadelphia, for June 1825.

† See Phil. Mag. vol. lxiv. p. 467; vol. lxv. p. 283.

with nitro-muriatic acid. By evaporating the liquor to dryness, and adding acidulated water and filtering, the silex was obtained. To the liquor from this operation ammonia in excess was added; and by this agent, the iron, manganese, and alumine contained in the liquor, were precipitated; the latter was separated from the two former by caustic potash. No attempt was made to ascertain the relative proportions of iron and manganese, this knowledge not being considered important. The presence of manganese was evidenced by the green colour of the alkaline fusion; and a rose-colour, when acid was added to the liquor. No allowance was made for the difference in the degree of oxidation of the iron and manganese in the substances used, and the products obtained, as the amount was less than one per cent, where most abundant.

The first experiments made with the globules were with potash and with carbonate of soda, on silver and on platina foil: with these agents I could not produce much effect; but by using a small quantity of carbonate of lime, carbonate of soda and borax, on platina foil, their fusion, whether they were coloured or colourless, opaque or transparent, was effected in a few minutes.

Experiment 1.—A piece of the purest anthracite of Lehigh, subjected to the blowpipe, presented numerous small white globules; few were tinged with violet, and two or three were blackish; the globules did not readily unite with one another: however, by long continued heat, some of the globules were obtained of the size of the head of a small pin; the greater number of them were but feebly translucent, and could be broken by a moderate force; others, though few in number, were transparent, hard, and not so brittle. The white globules were not magnetic, except when dark spots were present; the blackish ones were magnetic, and like the whole of them could be fractured by pressure. The surface of the mass whitened, as observed in the ordinary combustion of this coal, and presented veins or layers of the matter of the white globules, showing that the impurities of the coal were not regularly intermixed with its carbon, or, upon the supposition of its being fused carbon, that its production was extremely irregular.

With the flux before mentioned the different kinds of globules were melted without difficulty. By heating a centigramme and a half of the globules in powder, for a long time, with caustic potash, about 3-4ths of a centigramme of silex was obtained. It manifested itself by its gelatinous appearance before the water was driven off.

The

The result of the analysis of this anthracite was

Carbon			90·1
Water*			6·6
Residue by incineration, of a dirty white colour	3·3 consisting of	Silex	1·2
		Alumine	1·1
		Oxides of iron and manganese	0·2
		Loss	0·8
			100·0

Experiment 2.—The anthracite of Rhode Island, by the action of the blowpipe, presented a brownish appearance after cooling (owing to manganese). The surface exhibited numerous globules, larger than those of the Lehigh: some of them were transparent, colourless, and very brilliant by reflected light; others, and the most abundant, were black and opaque, and were strongly attracted by the magnet; a few were coloured white and black in spots, the white spots resembling enamel. The surface of the mass presented minute veins similar to those of the Lehigh.

Some of the black globules were heated for a long time on platina foil, with carbonate of soda; the mass was yellowish, but became black when immersed in water. By heating and dipping into water several times, the globules whitened: I could not effect their fusion in this way, but with the compound flux they readily fused. With this flux the different kinds were tried, and with the same effect.

The analysis of this anthracite from Rhode Island gave

Carbon			90·03
Water			4·90
Residue by incineration, which was of a light brick-red	5·07 consisting of	Silex	2·14
		Oxides of iron and manganese	2·50
		Loss	·43
			100·00

Another specimen from the same locality, whose colour was a little different, being of a deeper black, and which was not tried with the blowpipe, gave

Carbon			77·70
Water			6·70
Residue by incineration, colour the same as the former	15·60 consisting of	Silex	8·50
		Oxides of iron and manganese	7·10
		Alumine a trace	
			100·00

* It is rather singular that so great a quantity of water as is contained in anthracite should heretofore have escaped notice. It is my intention to examine all the different kinds of coal, to ascertain if this fact be general.

Experiment 3.—A specimen of plumbago from Borrowdale, of great purity, as judged by its external characters and mechanical properties, was subjected to the blowpipe: the globules began to form immediately and in great number, attended occasionally by scintillations, owing to the combustion of iron; the globules were small; the greater part of them were black, opaque, and of great lustre; others were dull, of a brownish colour, and feebly translucent: almost all of them were attracted by the magnet. The surface of the heated part of the plumbago was brownish.

The globules, though acted upon with great difficulty by soda, and by potash, readily yielded to the compound flux, and formed a limpid yellowish glass. A large globule, by repeatedly heating it with carbonate of soda and plunging it into water, became rough, and finally opened in the centre; it then dissolved in nitro-muriatic acid. By evaporating the liquor to dryness, the yellow colour of the iron was very manifest; acidulated water took it up, leaving a white substance like silex floating in the liquor.

The analysis of this plumbago gave

Carbon			88·37
Water			1·23
Residue by incineration, colour yellowish brick-red	10·4 consisting of	Silex	5·10
		Alumine	1·00
		Oxides of iron and manganese	3·60
		Loss	70
			100·00

Experiment 4.—An impure specimen of plumbago from the same locality gave numerous and large globules, some of the size of small shot; they readily formed: the majority of them were translucent, shining, and of a light greenish-yellow; others were dark-coloured; and some of them were dull externally. The dark globules, as well as the surface of the mass of plumbago exposed to the flame, were attracted by the magnet; some of the light-coloured ones were affected by the magnet, but only at the point where they had been attached to their support, owing to particles of the support adhering to them. During the combustion of the plumbago, there were occasionally scintillations; the heated surface of the mass was brownish.

A large globule of the lightest colour and magnetic only at one point, melted with ease when the compound flux was used; it formed a transparent mass when hot, and opaque and milky when cold. The black ones with the same flux were also fused; they were brownish when hot, and greenish when cold.

They

They were acted upon with great difficulty by caustic potash, and by carbonate of soda.

The analysis of this plumbago gave

Carbon			61·27
Water			5·33
Residue by incineration, colour of a dirty yellowish-red	33·4 consisting of	Silex	10·10
		Alumine	3·20
		Oxides of iron and manganese	20·00
		Loss	·10
			100·00

Experiment 5.—A specimen of plumbago remarkably pure, from near Bustletown, Pennsylvania, was tried with the blowpipe. The globules were formed with difficulty, probably owing to its foliated texture, the fused parts spreading over the surface. The colour in places was white and translucent, in others so dark as to be almost black.

With the flux before mentioned the fused matter was reduced to a transparent glass.

The analysis of this plumbago gave

Carbon			94·40
Water			0·60
Residue by incineration, colour light brick-red	5·0 consisting of	Silex	2·60
		Oxides of iron and manganese	1·40
		Loss	1·00
			100·00

Similar experiments were made with plumbago from several other localities; the results of which were nowise different, and therefore need no further mention.

Experiment 6.—A piece of charred mahogany, during its combustion by the compound blowpipe, presented numerous small imperfect globules, owing to the force of the flame, which dissipated their support before they had time to form or to accumulate to any considerable size: many of them adhered together, ramifying like *flos ferri*, which they resembled. They were collected by placing a dish under their support. By the compound flux they readily fused into a transparent glass.

Experiment 7.—A quantity of lampblack was pressed into a mould with great force, and made to assume the form of a cylinder of about 3-4ths of an inch in diameter and half an inch in thickness; it weighed seven grammes. This cylinder of lampblack was subjected to the blowpipe. It wasted away gradually, without forming any globules or fused matter visible

ble to the naked eye or to the microscope. The heat was equally as intense in this experiment as in all the other instances, and no condition was wanting to produce the same effects, except the difference of composition. After burning the lampblack for as long a time as was thought necessary to make the experiment a fair one, it was again weighed, and found to have lost four grammes $\frac{42}{100}$, for it weighed but two grammes $\frac{58}{100}$.

Five grammes of the same lampblack, heated in an open platina crucible, left after its incineration one centigramme of white ashes, equal to $\frac{1}{500}$ of the mass.

From the analyses of the substances used by Professor Silliman, from which the globules were obtained, it appears that they all contain foreign matter, as silex, iron, manganese, and some of them also alumine; that when lampblack was used which contained but $\frac{1}{500}$ of fixed impurities, no distinct globule or melted matter was formed, although the heat was sufficiently great, and the combustion slow enough to admit of the forming of globules, if their production was owing to the fusion of carbon, and not to extraneous matter. From my own experiments I always found that the more impure the substance was, the more numerous and the larger were the globules produced.

All the globules from the different kinds of substances used were readily fused by the compound flux, and underwent little change when it was not used; although the heat was, in this case, of longer continuance. Matter similar to the impurities discovered in the substances used was detected in them.

From these facts it would appear, that the globules produced from the combustible substances operated upon did not arise from the fusion of their carbon, since they can otherwise be accounted for; particularly as no experiment has been made which unequivocally leads to that conclusion. The experiment upon which Professor Silliman relies, as a proof of the globules being fused carbon, is one which is not satisfactory to me; if it had been, it would have given me great pleasure, for no one, I trust, feels more interested in the scientific prosperity of his country than I do; and if Professor Silliman were right, it would indeed be a triumph for America.

The experiment just alluded to (see Journal of Science, vol. vi. p. 347,) is the heating some of the coloured globules in oxygen gas by the solar rays, with a lens. The following is an extract from the papers.

"To detach any portion of unmelted plumbago which might adhere to them, I carefully rubbed them between my thumb and

and finger, in the palm of my hand. Although they were in the focus for nearly half an hour, they did not melt, diappear, or alter their form; it appeared, however, on examining the gas, that they had given up a part of their substance to the oxygen, for carbonic acid was formed which gave a decided precipitate with lime-water."

That this experiment is equivocal appears certain, as particles of the support might have been attached to the globules; for, from my own observations, I found that in a great number of instances, some of the white globules at the point of junction with their support had small dark particles attached to them; and when the surface from which they were detached was magnetic, they were attracted by the magnet when it was presented to those parts: I could not disengage those particles by rubbing the globules with my fingers against one another. It is very evident that, as the globules underwent no change (unless a reduction of volume, which is not mentioned), as the description clearly shows, the carbonic acid obtained might have been produced by the combustion of portions of the support adhering to them externally, and penetrating them to a certain extent.

In the experiment detailed in vol. v. p. 363, of the same Journal, the carbonic acid found probably had a similar origin, and the disappearance of the globules may have been owing to their incorporating themselves with the piece of brick upon which they were placed, as the brick was vitrified at the point where they were placed.

Professor Silliman seems disposed to lay great stress on the loss in my examination of the globule sent by Dr. Macneven. I thought I had well accounted for it, as the particle was small, action violent, and I merely wished to show chemically the presence of iron. I could not for one moment entertain the idea that carbon existed in it, in any notable proportion; for I know of no combination of iron and carbon, at common temperature, which could give a product possessed of the malleability and toughness which the globule possessed.

I was sorry to observe that Professor Silliman in his reply to my paper seems offended that I did not notice his communications upon the subject of these globules, particularly as the discovery was his, and was justly entitled to such consideration. My silence certainly appeared uncourtly; but it was not owing to ignorance of his labours, or a want of regard to him personally, or as a chemist; Professor Silliman's merit is too well known to be affected by me.

XXIII. *On*

XXIII. *On the Phænomena of Lunar Eclipses.* By Mr. M. Smith.

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN all works on astronomy with which I am acquainted, and in which the doctrine of lunar eclipses is treated on, there are two circumstances connected therewith which in my opinion have always been attributed to wrong causes: one of these is, that the apparent semidiameter of the earth's shadow is always greater by about 50 seconds than theory indicates; and the other is, that the intensity of the shadow is extremely variable, it sometimes rendering the moon entirely invisible, and at other times permitting her to be very distinctly seen, of a ruddy colour resembling tarnished copper.

With respect to the first of these circumstances, namely, the dimensions of the shadow, the rule for finding this, deduced from theory, is as follows. Add together the horizontal parallaxes of the sun and moon, from which subtract the sun's semidiameter, and the remainder will be the semidiameter of the earth's shadow. But as the semidiameter of the shadow is found by observation always to exceed this quantity by about 50 seconds, astronomers have directed this augmentation to be made to the computed magnitude; and not having discovered the true cause of this enlargement of the shadow, they have erroneously ascribed it to the earth's atmosphere.

It will be a sufficient refutation of this error to observe, that the earth's shadow could not be enlarged 50 seconds by such means, unless the atmosphere were sufficiently dense at a height of nearly sixty miles from the earth's surface to project a shadow: and this is a supposition which I believe no one will contend for; particularly when it is considered that the atmosphere, by refracting the sun's rays, must tend to diminish rather than increase the shadow. I shall now show that this enlargement of the shadow is occasioned by an obvious cause which has hitherto been unnoticed, and is totally independent of the earth having any atmosphere.

In the first place, it is to be particularly observed that the rule above given for computing the semidiameter of the shadow is founded on the assumption, that a spectator placed in the moon any where in the visible boundary of the shadow would see the limb of the sun and that of the earth in contact;—and in this lies the mistake. A spectator so placed would see a certain small portion of the sun's disc, amounting to about the 120th part of

of its surface, or about 50 seconds of its diameter; the reason of which will appear from the following consideration.

Suppose that during a partial lunar eclipse a curve were described on the lunar disc, exactly to pass through all those points in which the limbs of the sun and earth appear in contact: it is evident that all that portion of the lunar disc lying within that curve will have no solar light, and would therefore be invisible or eclipsed: it is further evident, that an infinitely small portion of solar light will not suffice to render the surface of the moon visible, but that a certain definite portion of light is necessary for that purpose. Let therefore another curve, exterior to the former and every where at the distance of 50 seconds from it, be described; and it may easily be conceived, that although the space situated between these two curves will be enlightened by a small portion of the solar rays, it will not be sufficiently illuminated to become visible by reflection; consequently, that this latter curve, and not the former, will be the visible boundary of the eclipse: and hence the shadow appears enlarged, by extending a small space into the penumbra. The precise quantity of this augmentation, perhaps, can only be determined by observation:—admitting it to be 50 seconds, it may be found by computation that the shadow appears to terminate where the light is reduced to about the 120th part of the whole. It may further be observed, that whatever be the amount of this augmentation, it may without sensible error be considered a constant quantity, the variation therefrom never amounting to more than a single second, being chiefly dependent on the semidiameter of the sun, and not on the parallax of the moon: it is, however, probable, that the augmentation may vary during the progress of an eclipse, being always greatest at the beginning and end, and least at the limits of total darkness, a smaller portion of light sufficing to render the moon's surface visible in the latter case than in the former.

To prove this augmentation by experiment, procure a cylinder of which the diameter is not less than an inch and a quarter, nor greater than an inch and a half. (The outer tube of a pocket telescope is a very convenient object for this purpose.) Measure the diameter of this cylinder very accurately, and on a card draw with a pencil two parallel lines about three or four inches in length, and distant from each other the diameter of the cylinder. Between these two lines draw two other lines parallel with the former, and each half an inch distant therefrom: you will then have four parallel lines and three spaces. Divide each of the two outer spaces into eight equal parts by means of seven lines all parallel with

the former: you will then have two sets of parallel lines (resembling staves for music), with a blank space between them equal to the excess of the diameter of the cylinder above an inch. Now place the cylinder upright in a window exposed to the sun, and at a distance of 107 inches*, or nearly nine feet therefrom: place the card so that the shadow of the cylinder may fall thereon; and it will be found that the shadow will fill all the central space, and also one of the sixteen small spaces, this small space being the amount of the augmentation of the shadow: for it is evident that if the shadow were projected from the centre of the sun, it would at all distances from the cylinder be equal thereto, and would therefore fill all the space between the two outer lines. If, on the other hand, the shadow were projected from the limb of the sun, it would be reduced one inch at the distance of 107 inches, and would consequently only fill the space between the two inner lines; but as it is found experimentally to exceed this magnitude by one of the sixteen small divisions, it proves that to an eye placed at the visible limit of the shadow, one sixteenth part of the sun's semidiameter would be seen. This quantity amounts exactly to one minute of a degree, which is consequently the augmentation of a shadow cast by any object of which the boundary is a right line. It appears, then, that the shadow of an object is projected from a point in the sun's disc one minute within his limb, and therefore fifteen minutes from his centre; which circumstance must be particularly attended to in the construction of sundials, for otherwise they will always err exactly one minute of time from the truth, being too fast in the forenoon and too slow in the afternoon, the shadow being cast from a point in the sun's disc one minute within that limb which is nearest to the meridian. As this is a circumstance not generally noticed by authors who have treated on dialling, it is highly probable that dials are often constructed without attention being paid to it; in which case, if a watch were set by such a dial in the forenoon, it would be found to vary two minutes from it in the afternoon.

The reason why the augmentation of the earth's shadow in a lunar eclipse falls short of one minute is, that the object which casts the shadow is not terminated by a right line, but by a curve; hence an equal proportion of the sun's disc will be uncovered with a smaller portion of his diameter. A due consideration of this point will prove that the augmentation of the shadow cannot be sensibly affected by any variation of

* Accurately 105·5 inches when the sun is in perigee, and 109 inches when he is in apogee; this being the number of diameters of the sun which he is distant from the earth.

the

the moon's parallax, inasmuch as that the object producing it (viz. the earth) would, if viewed from the moon, appear bounded by a curve, though of less convexity, as the parallax increases; hence the augmentation could not amount to a minute, although the parallax were increased to any extent: it may therefore, as before observed, be considered a constant quantity, or only affected by the variation of the sun's semidiameter.

Let us now consider the reason why the moon when totally eclipsed is on some occasions entirely invisible, while on other occasions she appears considerably illumined with a sort of ruddy light. The cause usually assigned for this variation is the different distances of the moon from the earth: but I can affirm from my own experience that this solution is unsatisfactory, having seen the eclipsed portion of the moon very distinctly both when she was near her apogee and her perigee. I apprehend that this variation will be found to depend entirely on them oon's declination; and that she will be always very visible when near the equator, and quite invisible when near either of the tropics. To explain the cause of this, let us inquire what appearance a lunar eclipse would present to a spectator in the moon. This lunarian observer would evidently have a total solar eclipse; and, on directing his view towards the place of the sun, he would see nothing but a luminous ring nearly two degrees in diameter, but so very slender as probably not to exceed a minute in thickness: this ring would be the effect of the earth's atmosphere refracting the rays of the sun; and its degree of brightness would, I imagine, be less than that of the sun when in the horizon. Now if the moon were vertically over the earth's equator, the two poles would be in the circumference of the earth's disc, and the luminous ring would be vastly brighter in those parts contiguous to the poles, by reason of the great refraction of the earth's atmosphere in the polar regions: if, on the contrary, the moon were in the tropic, one of the poles would be turned away from the moon, and the other would fall considerably within the disc; so that the luminous ring would not appear particularly bright in any part, and therefore would not illumine the moon sufficiently to render her visible at the earth. If this solution be admitted, it follows that the visibility of the moon in eclipses depends on the position of the lunar nodes, the moon being always most visible when the nodes coincide with the equinoctial points, and least visible when they are in the solstitial points. The eclipse of June 10, 1816, was an example of this,—although the air was very clear, the moon so entirely disappeared that when totally eclipsed her place could

not be discovered in the heavens. The eclipses of 1820, happening in March and September, were examples of a contrary kind, being very luminous. It is probable that the moon never entirely disappears, unless the eclipse occurs within a month of the solstice.

Leaving this hypothesis to be confirmed or refuted by future observation,

I remain, sir, yours, &c.

Aug. 22, 1825. M. Smith.

XXIV. *Further Remarks on the Dichotomous Distribution of Nature: together with a Binary Arrangement and Description of the Genus* Sedum. *By* A. H. Haworth, *Esq. F.L.S. &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

HAVING been informed since my last communication to you, by my friend Mr. J. E. Gray, that our great countryman Ray had used a method resembling my *binary* one, in arranging some of his works on Botany; I have searched and found (what I must before have seen and forgotten) that not only Ray, but his great coadjutor Willoughby likewise, has used, in various branches of nature, a tabular method which is *chiefly* binary, yet sometimes irregular and even trichotomous, and ever descriptive; whereas mine is always binary, and strongly and strictly definitive (not descriptive), by a single word or name, for every branch or group, however small. And I have further found that Morison also has closely followed the plan of Ray and Willoughby, and used it very extensively in his celebrated and most useful *Historia Plantarum Universalis;*—thus all together forming and advancing a more powerful plea in favour of the binary distribution than any thing that I can add, or than any of our cotemporaries can easily overthrow.

Greater names than the above, in the whole march of the science of Natural History (considering what had preceded), have scarcely been; and they have, separately or jointly, laid the foundations of much that is yet stable, and whose superstructure is worthy of enduring for ever.

I have the honour to remain, sir, yours, &c.

Queen's Elm, Chelsea, Aug. 1825. A. H. Haworth.

P. S. To render the above little article rather more worthy of your Magazine and Journal, I will subjoin to it a descriptive account

account of the Acute-leaved section of the genus *Sedum*, together with a corresponding tabular and binary view of the same; to which I have been able to add one apparently unrecorded species from the royal gardens of Kew.

The *specific names* in the binary table are distinguished by italics, and their essential characters are added in roman letters.

Hardly any group of plants of such small extent, and all European rock or wall plants too (except, perhaps, the third), has been so little understood as the Seda *acutifolia*, by the best and latest authors on the subject. Even Linnæus himself, whose definitions and descriptions are usually unrivaled, gives us (not very clearly) two species only in the last edition of his *Species Plantarum*,—viz. *S. reflexum*, and *S. rupestre*; and to the last of these attributes quinquefarious leaves, a character I have never been able to find in any *Sedum* whatever; and yet he cites *Sedum rupestre*, &c. of Dillen. *Hort. Elth.* p. 342, *cum icone*, which has multifarious leaves, and is the original *S. rupestre*.

Willdenow, in his edition of the *Species Plantarum*, added one species (*S. virens*), a Portuguese plant, from the *Hortus Kewensis*, and in his Enumeration of the Plants in the Royal Gardens of Berlin and in page 25 of its Supplement (which is called a more accurate account), several others. But these latter species were published after the death of Willdenow, by Schlechtendal, and very erroneously as to nomenclature, as will appear by the explanatory *synonyma* hereunder given.

Four species only of the *Seda acutifolia* occur in the second edition of the *Hortus Kewensis*, one of which is the abovementioned *S. virens* from Portugal. Four also, and all English, are delineated in English Botany. Seven are found in Link's enlarged Enumeration of the Plants in the Berlin Gardens, published A.D. 1821: and I now cultivate thirteen apparently distinct species, which I have endeavoured to distinguish from each other as follows.

Classis et Ordo. Decandria Pentagynia.

Genus, Sedum Auctorum (exclusâ Anacampserote *Raii* &c.)

* *Acutifolia* Nob.: foliis plùs minùs lineari-subulatis lævibus nudis, ferè mucronatim acutis, subtùs convexis; suprà planiusculis vel depressis vix turgidè tumescentibus: floribus cymosis.

† *Longi-*

† *Longifolia:* foliis confertis plùs minùs spiraliter septemfariis glaucis, longioribus quàm cæteris.

+– *Alba:* petalis albis.

ochroleucum. 1. S. (tall sharp-cupped) foliis longis lineari-sub-lanceolatis mucronulatis glauco-albis, calycibus acutis.

Sedum ochroleucum. *Sm. Fl. Gr. Prod.* 1. *p.* 312. et *Linn. Trans. v.* 10. *p.* 6, synonymis exclusis.

Habitat in Græciæ montibus.

Floret in hortis Jul. G. H. ♃.

Calycis foliola acuta, externè s. dorso subindè concava. *Petala* spatulato-oblonga inflexo-concava alba. *Squamula* ordinaria quadrata retusa minuta, lente solùm obvia sine colore.

Obs. Should be wintered, as the Prince De Salm Dyck assures me, in a frame or green-house, being much less hardy than the following species.

Jacquini. 2. S. (tall blunt-cupped) foliis sublineari-lanceolatis longis acutis glauco-albis, suprà concaviusculis: calycibus obtusis.

Sempervivum sediforme. *Jacq. Hort. Vind.* 1. *t.* 81. —*Misc. Nat.* 1. *t.* 5, *casualis varietas.*—Sedum altissimum. *Pl. Gr.* 40. et *Nob. in Synops. Pl. Succ.*

Habitat in Italiâ, &c.

Floret Julio. H. ♃.

Obs. Priore simillimum at fortè humilius, et differt foliis latioribus obtusioribus concavioribus, petalis albis latioribus, foliolis calycinis obtusioribus brevioribus subparabolicis, dorso concaviusculis. *Squamula* ordinaria quadrata retusa subemarginatave per lentem.

++ *Lutea:* petalis luteis.

cærulescens. 3. S. (great straw-flowered) foliis longis congestis patulis subulatis acutis glauco-cærulescentibus, suprà planiusculis.

Habitat ... Viget in horto regio Kewense. H. ♃.

Floret Julio.

Obs. Prioribus simile, at duplò minùs magis cærulescens, foliis distantioribus in juventute, magis incurvis. *Petala* acutiora minùs concava pallidè lutea potiúsve saturatè straminea. *Squamula* ordinaria quàm in priore duplò humilior, supernè rotundata, sive breviter parabolica. Nova species, et forsan exotica. Fortassè Americana. Vix *S. stenopetalum.* Pursh.

reflexum.

reflexum. 4. S. (great reflexing-leaved) foliis longis distantioribus subulatis acutis depressis, senectis flexuosè patenti-recurvis glaucis.

Sedum reflexum. *Linn. Sp. Pl.* 1. 618.—*Engl. Bot.* 695, bona, at sine foliis reflectentibus. Sedum minus hæmatodes. *Ger.* 413.

Habitat apud muros tectos, &c. frequens. H. ♃.

Floret Julio, petalis oblongo-lanceolatis saturatè luteis.

β. Sedum minus luteum, ramulis reflexis. *Raii Synops.* 217. Varietas prætrivialis.

†† *Brevifolia*, foliis brevioribus.

+– *Erectifolia*, foliis junioribus erectis.

++ *Glauca*, foliis spiraliter 7-fariis perglaucis.

septangulare. 5. S. (great glaucous 7-angled) foliis arctè septangulatim imbricantibus incurvis patentibus mediocribus glaucis. *Nob. in Synops. Pl. Succ.* 116.

Habitat in Europâ ad montium radices, at nondum in Britanniâ.

Floret Julio. H. ♃.

Obs. Priore proximum, at distinguitur foliis brevioribus vix subulatis, non reflexis et semper concinnè septemfariis. Ramosius etiam, ramis brevioribus. *Flores* lutei.

Sedum rupestre. *Pl. Gr. t.* 115, nec *Linnæi.*—Sedum collinum. *Willd. Enum. Suppl. p.* 25.

glaucum. 6. S. (large short-leaved glaucous) foliis congestis brevibus subulatis erectis perglaucis, ramulis subquadripollicaribus.

Sedum glaucum. *Nob. in Synops. Pl. Succ.*—*Engl. Bot.* 2477, at è plantâ foliis nimis distantibus.

Habitat in Angliâ, comitatu Suffolciensi, ad latera australiora collium. H. ♃.

Floret Junio, corollâ luteâ. Ordinaria *squamula* quadrata.

Obs. Florem unum et eum morientem solùm dissecavi.

minus. 7. S. (lesser short-leaved glaucous), foliis congestis brevibus subulatis erecto-incurvis perglaucis, ramulis abbreviatis.

Sedum reflexum. *Pl. Gr.* 116, vix aliorum.—Sedum glaucum, β, *minus*, ramis ramulisque magis compactis (quàm in priore) erectioribus 1—2-uncialibus. *Nob. in*

in Synops. Pl. Succ. 117.—Sedum rupestre. *Willd. Enum. Suppl. p.* 26, nec *Linnæi.*

Habitat ... Non in Britanniâ? H. ♃.

Cum priore floret.

spirale. 8. S. (small 7-angled glaucous) foliis concinnè spiraliter 7-fariis congestis brevibus subulatis incurvo-imbricatis glauco-subcærulescentibus.

Sedum sexangulare. *Pl. Grass. f.* 118. Icone e plantâ debili??

Habitat ... Non in Britanniâ.

Floret Junio. H. ♃. corollis luteis.

Obs. Distincta et concinna species, staturâ ferè prioris, at glaucior et florendi tempore cærulescens, foliis ferè semper distinctè 7-fariis. Accepi a Principe De Salm Dyck nomine *Sedum sexangulare*, horti Parisiensis.

Obs. Sequentem descriptionem alio tempore faciebam ab eodem radice.

Descriptio. Ramoso-cæspitosum, ramis 3—4-entalibus sæpe tortuosis procumbentibus pendulisve, gracilibus. *Folia* erecta spiraliter 7-faria densissimè imbricata subfarinoso-alba, s. pruinosa, subulato-teretia 3-linearia. *Caulis* floriferus semipedalis infernè flexuosus, *foliis* erectis tectus. *Cyma* parva erecta compacta densè foliosa, floribus luteis parvis.—*Squamula* ord. diaphana retuso-rhombea minutissima.

++++ *Viridia:* foliis virescentibus viridibusve.

recurvatum. 9. S. (slender-leaved lesser green) foliis subulatis gracilioribus suberectis glaucescentibus mucronulatis brevibus.

Sedum recurvatum, foliis tereti-subulatis mucronatis glaucescentibus, ramorum sterilium reflexis, floralium recurvato-patentibus, ramis cymæ recurvatis. *Willd. Enum. Suppl. p.* 25.—Sed non *S. reflexum* Linn. ut voluit *Willd. En. Sup.* 25, et *Link Enum. H. Berol. p.* 438.—S. recurvatum *Nob. in Revis. Pl. Succ.* 29.

Habitat ... Non in Angliâ.

Floret Julio. H. ♃.

Obs. Statura prioris, at foliis certè gracilioribus, glaucescentibus, vel virescentibus.

anopetalum. 10. S. (broader-leaved lesser green) foliis patentibus brevibus sublanceolato-linearibus, vel utrinque attenuatis viridibus.

Sedum anopetalum. *De Cand. Fl. Fran.* 6. 526 (quod opus

opus non vidi) secundum *Link in Enum. Hort. Berol.* 1. 438.—*Nob. in Revis. Pl. Succ. p.* 29.

Habitat in Galliâ australi. H. ♃.

Floret Junio, affinium more, corollis pallidè lutescentibus.

Obs. Est Sedum collinum *Nob. in Revis. Pl. Succ. p.* 29, excluso charactere specifico *ex Willd.* quod ad S. septangularem pertinet. Est etiam Sedum nutans *Nob.* in loco.

β, *aurantiacum:* corollis aurantiacis. Hæc varietas (forsanve species) accepi cum nomine *Sedum nutans* var. a Principe De Salm Dyck, A.D. 1821, at periit ob longum iter, et alibi non vidi.

virens. 11. S. (deep-green leaved) foliis distantioribus mediocribus saturatè viridibus subulatis incurvis.

Sedum virens. *Ait. Hort. Kew. ed.* 1 & 2. *vol.* 3. *p.* 114.— *Willd. Sp. Pl.* 2. 764. — Sedum reflexum. *Willd. Enum. Suppl.* 25.—*Nob. in Synops. Pl. Succ.* 116. Sedum crassicaule. *Link Enum. Hort. Berolin.* 1. 438.

Habitat in Lusitaniâ. H. ♃.

Floret Junio, corollis luteis.

Obs. Prioribus quinque majus. *Folia* incurvatim erecto-patula et longiora quàm in illis, non recurva.

++ *Rosularia* foliis brevibus confertissimè numerosioribus viridibus glaucescentibusve, et in apicibus ramorum incurvato-patentibus rosulasque sæpè formantibus.

rupestre. 12. S. (expanding glaucous) foliis in ramorum apicibus rosulas sæpe formantibus, glaucis.

Sedum rupestre repens foliis compressis. *Dill. Hort. Elth. fig.* 342.—S. rupestre, *Engl. Bot. t.* 170, è plantâ foliis minùs quàm frequens expansis, florendi tempore. —Vix Sedum rupestre *Linn. Sp. Pl.* 1. 618, foliis quinquefariam confertis; propter folia semper multifaria.

Sedum virescens. *Willd. Enum. Hort. Berolin. Suppl.* 25, et inde *Nob. in Revis. Pl. Succ. p.* 29.

Habitat in Angliæ rupibus. H. ♃.

Nullam *Sedi* speciem vidi foliis 5-fariis.

Forsteri. 13. S. (expanding green) foliis saturatè viridibus, in ramulorum apicibus rosulas semper formantibus.

Sedum Forsteri. *Engl. Bot. t.* 1882, bona.

Habitat in Cambriæ rupibus. H. ♃.

Floret Julio, corollis luteis.

Obs. It may be worth mentioning that all the above described plants, (save No. 1) are grown in proportionably large pots of common light unmanured garden earth; that they stand out of doors in the full sun, watered as freely as green-house plants during spring and summer, and that they are regularly shifted and pruned in September; and in winter every pot is laid down sideways, to keep out superfluous wet, and the head of each pointed to the south. In winter they are only watered occasionally. With such treatment hardy succulent plants succeed, and flower very well, and assume the short and natural habit of their species upon walls and native rocks. But when planted in the borders or garden rock-work, they often smother each other, or grow luxuriantly rude.

** *Obtusifolia.* (To be continued.)

SEDORUM SPECIES, TABULA DICHOTOMA.

SEDUM (exclusâ Anacampserote *Raii* &c.)

- Acutifolia, foliis subulatis acutis.
  - Longifolia, foliis quàm cæteris longioribus.
    - Alba, petalis albis.
      1. *ochroleucum*, calyce acuto.
      2. *Jacquini*, calyce obtuso.
    - Lutea, floribus luteis.
      3. *cærulescens*, petalis pallidè luteis.
      4. *reflexum*, petalis saturatè luteis.
         - α, majus.
         - β, minus.
  - Brevifolia, foliis brevioribus.
    - Erectifolia, foliis junioribus erectis.
      - Glauca, foliis perglaucis.
        5. *septangulare*, foliis minùs subulatis.
        6. *glaucum*, ultimo 3-plo minus.
        7. *minus*, penultimo 6-plo minus.
        8. *spirale*, foliaceis ramis gracilioribus.
      - Viridia, foliis virescentibus, viridibusve.
        9. *recurvatum*, foliis tenuioribus virescentibus.
        10. *anopetalum*, foliis viridibus erectioribus.
            - α, lutescens.
            - β, aurantiacum.
        11. *virens*, majus: foliis lætè viridibus patulis.
    - Rosularia, foliis junioribus sæpiùs rosaceis.
      12. *rupestre*, foliis glaucis.
      13. *Forsteri*, foliis viridibus semper rosaceis.
- Obtusifolia, ... [*To be continued when the species can be obtained.*]

Erratum in my last communication.
Page 28, line 10, *for* claviforme *read* claviformem.

XXV. *Reply*

XXV. *Reply to Mr.* Davies's *Critique on Mr.* Herapath. *By* A Correspondent.

To the Editor of the Philosophical Magazine and Journal.

Sir,

MR. DAVIES's remarks in your last Number on some of Mr. Herapath's mathematical writings are tinctured with so much good feeling, that I cannot believe the incorrect views he has given of Mr. H.'s demonstration of the binomial theorem could be occasioned by any thing but haste or misconception. It is however a fact, that his statement of Mr. Herapath's demonstration is by no means just; and as all his other observations are founded on this, I trust you will allow me a small space to do Mr. H.'s labours that justice they deserve.

After slightly commenting on Mr. Herapath's proof for positive integers, which is too much of the axiomatic kind to need a word in its defence, Mr. Davies says (Phil. Mag. for August 1825, p. 117): "Here, however, Mr. Herapath is obliged to pause, and to call in the aid of a new principle on which to found his inferences; viz. *that because it was true when* r *and* v *were integers, it must be true when they are fractions, and even when they are irrationals and imaginaries too!*" A little lower Mr. D. adds: "The investigation lays it down as an essential datum that r and v shall be *integers;* and immediately proceeds on the supposition of their being *fractions*, *irrationals*, or *imaginaries*, taken at pleasure!"

Now as soon as Mr. Herapath has demonstrated the binomial theorem for a positive integral power, which he denotes by n, the only exponent yet introduced, he proceeds to the consideration of non-integral powers, (Phil. Mag. for May 1825, p. 323,) thus

"*Again for non-integral exponents.*

"*Suppose the second, third, fourth, &c. coefficients of the* r*th power are represented by* 2_r, 3_r, 4_r," &c. A little lower Mr. H. says: "*multiplying the* r*th power by the* v*th, the actual operation gives*," &c. These are Mr. Herapath's introductions of the rth and vth indices; and certainly nothing is here implied of their being integers. He is in fact distinctly speaking of "non-integral exponents," and at the very beginning of the next page recalls our attention to the non-integral signification of r and v, in the following terms: "*Now, if* n = r + v, *and* n *be as before an integer*," (Mr. H., as I have said, had previously employed n alone in his investigation for the inte-

 ger

ger exponent) " r *or* v *may be any number rational, irrational, or imaginary*," &c.

That these quotations contain all that Mr. Herapath says of the separate numerical values of r and v, any one may satisfy himself by a reference to the pages in question; and that they have not the most *distant* allusion to r and v having at any time integer values, evidently requires not a word to show. Nothing more therefore need be added in answer to Mr. Davies's statements;—that Mr. Herapath "has deceived himself,"—that his "investigation lays it down as an essential datum that r and v shall be *integers;* and immediately proceeds on the supposition of their being *fractions*, *irrationals*, or *imaginaries* taken at pleasure!" &c. &c. &c.

With respect to the accuracy of Mr. Herapath's mathematical discoveries and labours in other parts of his writings (most of which it seems Mr. Davies would overwhelm with the ruins of the binomial demonstration), the best proofs are the examples Mr. H. has generally interspersed; to which I beg, therefore, to refer your readers.

I have the honour to be, sir, yours, &c.

P. Q.

XXVI. *On new Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat.* By M. Faraday, *F.R.S. Cor. Mem. Royal Academy of Sciences of Paris, &c.**

THE object of the paper which I have the honour of submitting at this time to the attention of the Royal Society, is to describe particularly two new compounds of carbon and hydrogen, and generally, other products obtained during the decomposition of oil by heat. My attention was first called to the substances formed in oil at moderate and at high temperatures, in the year 1820; and since then I have endeavoured to lay hold of every opportunity for obtaining information on the subject. A particularly favourable one has been afforded me lately through the kindness of Mr. Gordon, who has furnished me with considerable quantities of a fluid obtained during the compression of oil gas, of which I had some years since possessed small portions, sufficient to excite great interest, but not to satisfy it.

It is now generally known, that in the operations of the Portable Gas Company, when the oil gas used is compressed in the vessels, a fluid is deposited, which may be drawn off and

* From the Philosophical Transactions, vol. x. part i.

preserved

preserved in the liquid state. The pressure applied amounts to 30 atmospheres; and in the operation, the gas previously contained in a gasometer over water, first passes into a large strong receiver, and from it, by pipes, into the portable vessels. It is in the receiver that the condensation principally takes place; and it is from that vessel that the liquid I have worked with has been taken. The fluid is drawn off at the bottom by opening a conical valve: at first a portion of water generally comes out, and then the liquid. It effervesces as it issues forth; and by the difference of refractive power it may be seen that a dense transparent vapour is descending through the air from the aperture. The effervescence immediately ceases; and the liquid may be readily retained in ordinary stoppered, or even corked bottles, a thin phial being sufficiently strong to confine it. I understand that 1000 cubical feet of good gas yield nearly one gallon of the fluid.

The substance appears as a thin light fluid; sometimes transparent and colourless, at others opalescent, being yellow or brown by transmitted, and green by reflected light. It has the odour of oil gas. When the bottle containing it is opened, evaporation takes place from the surface of the liquid; and it may be seen by the striæ in the air that vapour is passing off from it. Sometimes in such circumstances it will boil, if the bottle and its contents have had their temperature raised a few degrees. After a short time this abundant evolution of vapour ceases, and the remaining portion is comparatively fixed.

The specific gravity of this substance is 0·821. It does not solidify at a temperature of 0° F. It is insoluble, or nearly so, in water; very soluble in alcohol, æther, and volatile and fixed oils. It is neutral to test colours. It is not more soluble in alkaline solutions than in water; and only a small portion is acted upon by them. Muriatic acid has no action upon it. Nitric acid gradually acts upon it, producing nitrous acid, nitric oxide gas, carbonic, and sometimes hydrocyanic acid, &c., but the action is not violent. Sulphuric acid acts upon it in a very remarkable and peculiar manner, which I shall have occasion to refer to more particularly presently.

This fluid is a mixture of various bodies; which, though they resemble each other in being highly combustible, and throwing off much smoke when burnt in large flame, may yet by their difference of volatility be separated in part from each other. Some of it drawn from the condenser, after the pressure had been repeatedly raised to 30 atmospheres, and at a time when it was at 28 atmospheres, then introduced rapidly into a stoppered bottle and closed up, was, when brought home,

home, put into a flask and distilled, its temperature being raised by the hand. The vapour which came off, and which caused the appearance of boiling, was passed through a glass tube at 0°, and then conducted to the mercurial trough; but little uncondensed vapour came over, not more than thrice the bulk of the liquid; a portion of fluid collected in the cold tube, which boiled and evaporated when the temperature was allowed to rise; and the great bulk of the liquid which remained might now be raised to a comparatively high point, before it entered into ebullition.

A thermometer being introduced into another portion of the fluid, heat was applied, so as to keep the temperature just at the boiling point. When the vessel containing it was opened, it began to boil at 60° F. As the more volatile portions were dissipated, the temperature rose: before a tenth part had been thrown off, the temperature was above 100°. The heat continued gradually to rise, and before the substance was all volatilized it had attained 250°.

With the hope of separating some distinct substances from this evident mixture, a quantity of it was distilled, and the vapours condensed at a temperature of 0° into separate portions, the receiver being changed with each rise of 10° in the retort, and the liquid retained in a state of incipient ebullition. In this way a succession of products were obtained, but they were by no means constant; for the portions, for instance, which came over when the fluid was boiling from 160° to 170°, when re-distilled began to boil at 130°, and a part remained which did not rise under 200°. By repeatedly rectifying all these portions, and adding similar products together, I was able to diminish these differences of temperature, and at last bring them more nearly to resemble a series of substances of different volatility. During these operations I had occasion to remark, that the boiling point was more constant at, or between 176° and 190°, than at any other temperature, large quantities of fluid distilling over without any change in the degree; whilst in other parts of the series it was constantly rising. This induced me to search in the products obtained between these points for some definite substance; and I ultimately succeeded in separating a new compound of carbon and hydrogen, which I may by anticipation distinguish as bi-carburet of hydrogen.

Bi-carburet of Hydrogen.

This substance was obtained in the first instance in the following manner. Tubes containing portions of the above rectified products were introduced into a freezing mixture at 0°; many

many of them became turbid, probably from the presence of water; one, received at 176° (by which is meant that that was the boiling point of the contents of the retort when it came over), became partly solid, crystals forming round the side, and a fluid remaining in the centre; whilst two other portions, one received at 186° and the other at 190°, became quite hard. A cold glass rod being introduced into one of these tubes, the mass within was found to resist considerable pressure; but by breaking it down, a solid part was thrust to the bottom of the tube, whilst a fluid remained above: the fluid was poured off, and in this way the solid portion partly purified. The contents of the tube were then allowed to fuse, were introduced into a larger and stronger tube, furnished with another which entered loosely within it, both being closed of course at the lower end; then again lowering the temperature of the whole to 0°, bibulous paper was introduced, and pressed on to the surface of the solid substance in the large tube by the end of the smaller one. In this way much fluid was removed by successive portions of paper, and a solid substance remained, which did not become fluid until raised to 28° or 29°. To complete the separation of the permanently fluid part, the substance was allowed to melt, then cast into a cake in a tin-foil mould, and pressed between many folds of bibulous paper in a Bramah's press, care having been taken to cool the paper, tin-foil, flannel, boards, and other things used, as near to 0° as possible, to prevent solution of the solid substance in the fluid part to be removed. It was ultimately distilled from off caustic lime, to separate any water it might contain.

The general process, which appears to me to be the best for the preparation of this substance only, is to distil a portion of the fluid deposited during the condensation of oil gas, to set aside the product obtained before the temperature rises to 170°, to collect that which comes over by 180°, again separately that which comes over by 190°, and also the portion up to 200° or 210°. That before 170° will upon re-distillation yield portions to be added to those of 180° and 190°; and the part obtained from 190° upwards will also, when re-distilled, yield quantities boiling over at 180°, 190°, &c. Having then these three portions obtained at 180°, 190°, and 200°, let them be rectified one after the other, and the products between 175° and 195° received in three or four parts at successive temperatures. Then proceed with these as before described.

It will sometimes happen, when the proportion of bi-carburet of hydrogen is small in the liquid, that the rectifications must be many times repeated before the fluids at 185° and

190°

190° will deposit crystals on cooling; that is to say, before sufficient of the permanently fluid part at low temperatures has been removed, to leave a solution so saturated as to crystallize at 0°.

Bi-carburet of hydrogen appears in common circumstances as a colourless transparent liquid, having an odour resembling that of oil gas, and partaking also of that of almonds. Its specific gravity is nearly 0·85 at 60°. When cooled to about 32° it crystallizes, becoming solid; and the portions which are on the sides of the glass exhibit dendritical forms. By leaving tubes containing thin solid films of it in ice-cold water, and allowing the temperature to rise slowly, its fusing point was found to be very nearly 42° F.; but when liquid it may, like water and some saline solutions, be cooled much below that point before any part becomes solid. It contracts very much on congealing, 9 parts in bulk becoming 8 very nearly; hence its specific gravity in that state is about 0·956. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf-sugar.

It evaporates entirely when exposed to the air. Its boiling point in contact with glass is 186°. The specific gravity of its vapour, corrected to a temperature of 60°, is nearly 40, hydrogen being 1; for 2·3 grains became 3·52 cubic inches of vapour at 212°. Barometer 29·98. Other experiments gave a mean approaching very closely to this result.

It does not conduct electricity.

This substance is very slightly soluble in water; very soluble in fixed and volatile oils, in æther, alcohol, &c.; the alcoholic solution being precipitated by water. It burns with a bright flame and much smoke. When admitted to oxygen gas, so much vapour rises as to make a powerfully detonating mixture. When passed through a red-hot tube it gradually deposits carbon, yielding carburetted hydrogen gas.

Chlorine introduced to the substance in a retort exerted but little action until placed in sun-light, when dense fumes were formed, without the evolution of much heat; and ultimately much muriatic acid was produced, and two other substances, one a solid crystalline body, the other a dense thick fluid. It was found by further examination, that neither of these were soluble in water; that both were soluble in alcohol—the liquid readily, the solid with more difficulty. Both of them appeared to be triple compounds of chlorine, carbon, and hydrogen;—but I reserve the consideration of these, and of other similar compounds, to another opportunity.

Iodine appears to exert no action upon the substance in several

several days in sun-light; it dissolves in the liquid in small quantity, forming a crimson solution.

Potassium heated in the liquid did not lose its brilliancy, or exert any action upon it, at a temperature of 186°.

Solution of alkalis, or their carbonates, had no action upon it.

Nitric acid acted slowly upon the substance and became red, the fluid remaining colourless. When cooled to 32°, the substance became solid and of a fine red colour, which disappeared upon fusion. The odour of the substance with the acid was exceedingly like that of almonds, and it is probable that hydrocyanic acid was formed. When washed with water, it appeared to have undergone little or no change.

Sulphuric acid added to it over mercury exerted a moderate action upon it, little or no heat was evolved, no blackening took place, no sulphurous acid was formed; but the acid became of a light yellow colour, and a portion of a clear colourless fluid floated, which appeared to be a product of the action. When separated, it was found to be bright and clear, not affected by water or more sulphuric acid, solidifying at about 34°, and being then white, crystalline, and dendritical. The substance was lighter than water, soluble in alcohol, the solution being precipitated by a small quantity of water, but becoming clear by great excess*.

With regard to the composition of this substance, my experiments tend to prove it a binary compound of carbon and hydrogen, two proportionals of the former element being united to one of the latter. The absence of oxygen is proved by the inaction of potassium, and the results obtained when passed through a red-hot tube.

* The action of sulphuric acid on this and the other compounds to be described is very remarkable. It is frequently accompanied with heat; and large quantities of those bodies which have elasticity enough to exist as vapours when alone at common pressures, are absorbed. No sulphurous acid is produced; nor when the acid is diluted does any separation of the gas, vapour, or substance take place, except of a small portion of a peculiar product resulting from the action of the acid on the substances, and dissolved by it. The acid combines directly with carbon and hydrogen; and I find when united with bases forms a peculiar class of salts, somewhat resembling the sulphovinates, but still different from them. I find also that sulphuric acid will condense and combine with olefiant gas, no carbon being separated, or sulphurous or carbonic acid being formed; and this absorption has in the course of 18 days amounted to 84·7 volumes of olefiant gas to 1 volume of sulphuric acid. The acid produced combines with bases, &c., forming peculiar salts, which I have not yet had time, but which it is my intention, to examine, as well as the products formed by the action of sulphuric acid on naphtha, essential oils, &c., and even upon starch and lignine, in the production of sugar, gum, &c., where no carbonization takes place, but where similar results seem to occur.

The following is a result obtained when it was passed in vapour over heated oxide of copper:—0·776 grain of the substance produced 5·6 cubic inches of carbonic acid gas, at a temperature of 60°, and pressure 29·98 inches; and 0·58 grain of water were collected. The 5·6 cubic inches of gas are equivalent to 0·711704 grain of carbon by calculation, and the 0·58 grain of water to 0·064444 of hydrogen.

Carbon	0·711704	or 11·44
Hydrogen	0·064444	or 1·

These quantities nearly equal in weight the weight of the substance used; and making the hydrogen 1, the carbon is not far removed from 12, or two proportionals.

Four other experiments gave results all approximating to the above. The mean result was 1 hydrogen, 11·576 carbon.

Now considering that the substance must, according to the manner in which it was prepared, still retain a portion of the body boiling at 186°, but remaining fluid at 0°, and which substance I find, as will be seen hereafter, to contain less carbon than the crystalline compound (only about 8·25 to 1 of hydrogen), it may be admitted, I think, that the constant though small deficit of carbon found in the experiments is due to the portion so retained; and that the crystalline compound would, if pure, yield 12 of carbon for each 1 of hydrogen, or two proportionals of the former element and one of the latter.

2 proportionals carbon . 12
1 ——— hydrogen 1 } 13 bi-carburet of hydrogen.

This result is confirmed by such data as I have been able to obtain by detonating the vapour of the substance with oxygen. Thus in one experiment 8092 mercury grain measures of oxygen at 62° had such quantity of the substance introduced into it as would entirely rise in vapour; the volume increased to 8505: hence the vapour amounted to 413 parts, or $\frac{1}{20\cdot6}$ of the mixture nearly. Seven volumes of this mixture were detonated in an eudiometer tube by an electric spark, and diminished in consequence nearly to 6·1: these, acted upon by potash, were further diminished to 4, which were pure oxygen. Hence 3 volumes of mixture had been detonated, of which nearly 0·34 was vapour of the substance, and 2·65 oxygen. The carbonic acid amounted to 2·1 volumes, and must have consumed an equal bulk of oxygen gas; so that 0·55 remain as the quantity of oxygen which has combined with the hydrogen to form water, and which with the 0·34 of vapour nearly make the diminution of 0·9.

It will be seen at once that the oxygen required for the carbon is four times that for the hydrogen; and that the whole

statement

statement is but little different from the following theoretical one, deduced partly from the former experiments:—1 volume of vapour requires 7·5 volumes of oxygen for its combustion; 6 of the latter combine with carbon to form 6 of carbonic acid, and the 1·5 remaining combine with hydrogen to form water. The hydrogen present therefore in this compound is equivalent to 3 volumes, though condensed into one volume in union with the carbon; and of the latter elements there are present six proportionals, or 36 by weight. A volume therefore of the substance in vapour contains

Carbon	6 × 6	= 36
Hydrogen	1 × 3	= 3
		39

and its weight or specific gravity will be 39, hydrogen being 1. Other experiments of the same kind gave results according with these.

Among the liquid products obtained from the original fluid was one which, procured as before mentioned, by submitting to 0° the portion distilling over at 180° or 190°, corresponded with the substance already described, as to boiling points, but differed from it in remaining fluid at low temperatures; and I was desirous of comparing the two together. I had no means of separating this body from the bi-carburet of hydrogen, of which it would of course be a saturated solution at 0°. Its boiling point was very constantly 186°. In its general characters of solubility, combustibility, action of potassium, &c., it agreed with the substance already described. Its specific gravity was 0·86 at 60°. When raised in vapour 1·11 grain of it gave 1·573 cubic inch of vapour at 212°, equal to 1·212 cubic inch at 60°. Hence 100 cubic inches would weigh about 91·6 grains, and its specific gravity would be 43·25 nearly. In another experiment, 1·72 grain gave 2·4 cubic inches at 212°, equal to 1·849 cubic inch at 60°; from which the weight of 100 cubic inches would be deduced as 93 grains; and its specific gravity to hydrogen as 44 to 1. Hence probably the reason why, experimentally, the specific gravity of bi-carburet of hydrogen in vapour was found higher, than by theory it would appear to be when pure.

Sulphuric acid acted much more powerfully upon this substance than upon the bi-carburet: great heat was evolved, much discolouration occasioned, and a separation took place into a thick black acid, and a yellow lighter liquid, resisting any further action at common temperatures.

0·64 grain of this substance was passed over heated oxide of copper: 4·51 cubic inches of carbonic acid gas were obtained,

tained, and 0·6 grain of water. The carbonic acid and water are equivalent to

Carbon	0·573176,	or 8·764
Hydrogen . . .	0·066666	1·

but as the substance must have contained much bi-carburet of hydrogen, it is evident that, if in a pure state, the carbon would fall far short of the above quantity, and the compound would approximate of course to a simple carburet of hydrogen containing single proportionals.

New Carburet of Hydrogen.

Of the various other products from the condensed liquor, the next most definite to the bi-carburet of hydrogen appears to be that which is most volatile. If a portion of the original liquid be warmed by the hand or otherwise, and the vapour which passes off be passed through a tube at 0°, very little uncondensed vapour will go on to the mercurial trough; but there will be found after a time a portion of fluid in the tube, distinguished by the following properties. Though a liquid at 0°, it upon slight elevation of temperature begins to boil, and before it has attained 32° is all resolved into vapour or gas, which may be received and preserved over murcury.

This gas is very combustible, and burns with a brilliant flame. The specific gravity of the portion I obtained was between 27 and 28, hydrogen being 1: for 39 cubic inches introduced into an exhausted glass globe were found to increase its weight 22·4 grains at 60° F., bar. 29·94. Hence 100 cubic inches weigh nearly 57·44 grains.

When cooled to 0° it condensed again; and inclosed in this state in a tube of known capacity, and hermetically sealed up, the bulk of a given weight of the substance at common temperatures was ascertained. This compared with water gave the specific gravity of the liquid as 0·627 at 54°. It is therefore among solids or liquids the lightest body known.

This gas or vapour when agitated with water is absorbed in small quantities. Alcohol dissolves it in large quantity; and a solution is obtained, which, upon the addition of water, effervesces, and a considerable quantity of the gas is liberated. The alcoholic solution has a peculiar taste, and is neutral to test papers.

Olive oil dissolves about six volumes of the gas.

Solution of alkali does not affect it; nor does muriatic acid.

Sulphuric acid condenses the gas in very large quantity, 1 volume of the acid condensing above 100 volumes of the vapour. Sometimes the condensation is perfect; at other times a small quantity of residual gas is left, which burns with a pale

pale blue flame, and seems to be a product of too rapid action. Great heat is produced during the action; no sulphurous acid is formed; the acid is much blackened, has a peculiar odour, and upon dilution generally becomes turbid, but no gas is evolved. A permanent compound of the acid with carbon and hydrogen is produced, and enters as before mentioned into combination with bases.

A mixture of 2 volumes of this vapour with 14 volumes of pure oxygen was made, and a portion detonated in an eudiometer tube. 8·8 volumes of the mixture diminished by the spark to 5·7 volumes, and these by solution of potash to 1·4 volume, which were oxygen. Hence 7·4 volumes had been consumed, consisting of

Vapour of substance	1·1
Oxygen	6·3
Carbonic acid formed	4·3
Oxygen in carbonic acid	4·3
Oxygen combining with hydrogen .	2·0
Diminution by spark	3·1

This is nearly as if 1 volume of the vapour or gas had required 6 volumes of oxygen, had consumed 4 of them in producing 4 of carbonic acid gas, and had occupied the other 2 by 4 of hydrogen to form water. Upon which view, 4 volumes or proportionals of hydrogen = 4, are combined with 4 proportionals of carbon = 24, to form one volume of the vapour, the specific gravity of which would therefore be 28. Now this is but little removed from the actual specific gravity obtained by the preceding experiments; and knowing that this vapour must contain small portions of other substances in solution, there appears no reason to doubt that, if obtained pure, it would be found thus constituted.

As the proportions of the elements in this vapour appear to be the same as in olefiant gas, it became interesting to ascertain whether chlorine had the same action upon it as on the latter body. Chlorine and the vapour were therefore mixed in an exhausted retort: rapid combination took place, much heat was evolved, and a liquor produced resembling hydrochloride of carbon, or the substance obtained by the same process from olefiant gas. It was transparent, colourless, and heavier than water. It had the same sweet taste, but accompanied by an after aromatic bitterness, very persistent. Further, it was composed of nearly equal volumes of the vapour and chlorine: it could not therefore be the same as the hydro-chloride of carbon from olefiant gas, since it contained twice as much carbon and hydrogen. It was therefore treated with excess of chlorine in sun-light: action slowly took place,

more

more chlorine combined with the substance, muriatic acid was formed, and ultimately a fluid tenacious triple compound of chlorine, carbon, and hydrogen was obtained; but no chloride of carbon. This is a remarkable circumstance, and assists in showing that though the elements are the same, and in the same proportions as in olefiant gas, they are in a very different state of combination.

The tension of the most volatile part of the condensed oil gas liquid, and indeed of the substance next beneath olefiant gas in elasticity existing in the mixture constituting oil gas, appears to be equal to about 4 atmospheres at the temperature of 60°. To ascertain this a tube was prepared, like the one* delineated in the sketch, fig. 1, containing a mercurial

Fig. 1.

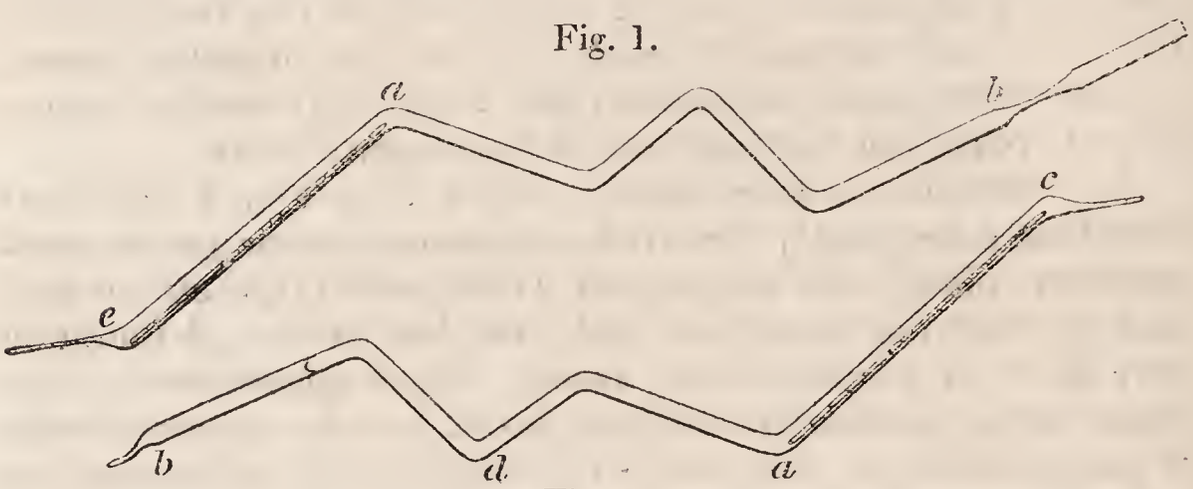


Fig. 2.

gauge at *a*, *c*, and the extremities being open. It was then cooled to 0° from *a* to *b*, and in that state made the receiver into which the first product from a portion of the original fluid was distilled. The part at *b* was then closed by a spirit lamp; and having raised enough vapour to make it issue at *c*, that was also closed. The instrument now placed as at fig. 2 had *a* and *d* cooled to 0°, whilst the fluid collected in *b* was warmed by the hand or the air; and when a portion had collected in *d* sufficient for the purpose, the whole instrument was immersed in water at 60°; and before the vapour had returned and been all dissolved by the liquid at *b*, the pressure upon the gauge within was noted. Sometimes the fluid at *d* was rectified by warming that part of the tube, and cooling *a* only, the re-absorption at *b* being prevented or rather retarded in consequence of the superior levity of the fluid at *d*; so that the first portions which returned to *b* lay upon it in a stra-

* The particular inclination of the parts of the tube one to another was given, that the fluid, when required, might be returned from *a* to *d* without passing on to *b*.

tum,

tum, and prevented sudden solution in the mass below. This difference in specific gravity was easily seen upon agitation, in consequence of the striæ produced during the mixture.

Proceeding in this way it was found, as before stated, that the highest elastic power that could be obtained from the substances in the tube was about 4 atmospheres at 60°; and as there seems no reason to doubt but that portions of the most volatile substances in oil gas beneath olefiant gas were contained in the fluid, inasmuch as even olefiant gas itself is dissolved by it in small proportions, it may be presumed that there is no substance in oil gas much more volatile than the one requiring a pressure of 4 atmospheres at 60°, except the well known compounds; or, in other words, that there is not a series of substances passing upwards from this body to olefiant gas, and possessing every intermediate degree of elasticity, as there seems to be from this body downwards, to compounds requiring 250° or 300° for their ebullition.

In reference to these more volatile products, I may state that I have frequently observed a substance come over in small quantity, rising with the vapour which boils off at 50° or 60°, and crystallizing in spiculæ in the receiver at 0°. A temperature of 8° or 10° causes its fusion and disappearance. It is doubtless a peculiar and definite body, but the quantity is extremely small, or else it is very soluble in the accompanying fluids. I have not yet been able to separate it, or examine it minutely.

I ventured some time since upon the condensation of various gases*, to suggest the possibility of forming a vapour lamp, which, containing a brilliantly combustible substance (liquid at a pressure of two, three, or four atmospheres at common temperatures, but a vapour at less pressure), should furnish a constant light for a length of time, without requiring high, or involving inconstant, pressure. Such a lamp I have now formed, feeding it with the substance just described; and though at present it is only a matter of curiosity, and perhaps may continue so, yet there is a possibility that processes may be devised, by which the substance may be formed in larger quantity, and render an application of this kind practically useful.

On the remaining Portions of the condensed Oil Gas Liquor.

It has been before mentioned, that by repeated distillations various products were obtained, boiling within limits of temperature which did not vary much, and which when distilled were not resolved into other portions, differing far from each

* Quarterly Journal of Science, xvi. 240.

other

other in volatility, as always happened in the earlier distillations. Though conscious that these were mixtures, perhaps of unknown bodies, and certainly in unknown proportions, yet experiments were made on their composition by passing them over oxide of copper, in hopes of results which might assist in suggesting correct views of their nature. They all appeared to be binary compounds of carbon and hydrogen, and the following table exhibits the proportions obtained: the first column expressing the boiling temperature at which the products were distilled, as before mentioned; the second the hydrogen, made a constant quantity; and the third the carbon.

140°	1	7·58
150°	1	8·38
160°	1	7·90
176°	1	8·25
190°	1	8·76
200°	1	9·17
210°	1	8·91
220°	1	8·46

These substances generally possess the properties before described, as belonging to the bi-carburet of hydrogen. They all resist the action of alkali, even that which requires a temperature above 250° for its ebullition; and in that point are strongly distinguished from the oils from which they are produced. Sulphuric acid acts upon them instantly with phænomena already briefly referred to.

Dr. Henry, whilst detailing the results of his numerous and exact experiments in papers laid before the Royal Society, mentions in that read February 22, 1821 *, the discovery made by Mr. Dalton, of a vapour in oil gas of greater specific gravity than olefiant gas, requiring much more oxygen for its combustion, but yet condensable by chlorine. Mr. Dalton appears to consider all that was condensable by chlorine as a new and constant compound of carbon and hydrogen; but Dr. Henry, who had observed that the proportion of oxygen required for its combustion varied from 4·5 to 5 volumes, and the quantity of carbonic acid produced, from 2·5 to 3 volumes, was inclined to consider it as a mixture of the vapour of a highly volatile oil with the olefiant and other combustible gases: and he further mentions, that naphtha in contact with hydrogen gas will send up such a vapour; and that he has been informed, that when oil gas was condensed in Gordon's lamp, it deposited a portion of highly volatile oil.

* Phil. Trans. for 1821, part i. See Phil. Mag. vol. lvii. p. 303.

A writer

A writer in the Annals of Philosophy, N. S. iii. 37, has deduced from Dr. Henry's experiments, that the substance, the existence of which was pointed out by Mr. Dalton, was not a new gas *sui generis*, "but a modification of olefiant gas, constituted of the same elements as that fluid, and in the same proportions; with this only difference, that the compound atoms are triple instead of double:" and Dr. Thomson has adopted this opinion in his Principles of Chemistry. This, I believe, is the first time that two gaseous compounds have been supposed to exist, differing from each other in nothing but density; and though the proportion of 3 to 2 is not confirmed, yet the more important part of the statement is, by the existence of the compound described at page 188; which, though composed of carbon and hydrogen in the same proportion as in olefiant gas, is of double the density *.

It is evident that the vapour observed by Mr. Dalton and Dr. Henry must have contained not only this compound, and a portion of the bi-carburet of hydrogen, but also portions of the other (as yet apparently indefinite) substances; and there can be no doubt that the quantity of these vapours will vary from the point of full saturation of the gas, when standing over water and oil, to unknown, but much smaller, propor-

* In reference to the existence of bodies composed of the same elements and in the same proportions, but differing in their qualities, it may be observed, that now we are taught to look for them, they will probably multiply upon us. I had occasion formerly to describe a compound of olefiant gas and iodine (Phil. Trans. cxi. 72), which upon analysis yielded one proportional of iodine, two proportionals of carbon, and two of hydrogen. (Quarterly Journal, xiii. 429.) M. Serrulas, by the action of potassium upon an alcoholic solution of iodine, obtained a compound decidedly different from the preceding in its properties; yet when analysed, it yielded the same elements in the same proportions. (*Ann. de Chimie*, xx. 245; xxii. 172.)

Again: MM. Liebig and Gay-Lussac, after an elaborate and beautiful investigation of the nature of fulminating compounds of silver, mercury, &c., were led to the conclusion that they were salts, containing a new acid, and owed their explosive powers to the facility with which the elements of this acid separated from each other. (*Annales de Chimie*, xxiv. 294; xxv. 285.) The acid itself, being composed of one proportional of oxygen, one of nitrogen, and two of carbon, is equivalent to a proportional of oxygen + a proportional of cyanogen, and is therefore considered as a true cyanic acid. But M. Wohler, by deflagrating together a mixture of ferro-prussiate of potash and nitre, has formed a salt, which, according to his analysis, is a true cyanate of potash. The acid consists of one proportional of oxygen, one of nitrogen, and two of carbon. It may be transferred to various other bases; as the earths, the oxides of lead, silver, &c.: but the salts formed have nothing in common with the similar salts of MM. Liebig and Gay-Lussac, except their composition. (Gilbert's *Annalen*, lxxviii. 157 *Ann. de Chimie*, xxvii. 190.) M. Gay-Lussac observes, that if the analysis be correct, the difference can only be accounted for by admitting a different mode of combination.

tions. It is therefore an object in the analysis of oil and coal gas, to possess means by which their presence and quantity may be ascertained; and this I find may be done with considerable exactness by the use of sulphuric acid, oil, &c., in consequence of their solvent power over them.

Sulphuric acid is in this respect a very excellent agent. It acts upon all these substances instantly, evolving no sulphurous acid; and though when the quantity of substance is considerable as compared with the acid, a body is left undecomposed by or uncombined with the acid, and volatile, so as constantly to afford a certain portion of vapour; yet when the original substance is in small quantity, as where it exists in vapour in a given volume of gas, this does not interfere, in consequence of the solubility of the vapour of the new compound produced by the action of the acid in the acid itself in small quantities: and I found that when 1 volume of the vapour of any of the products of the oil gas liquor was acted upon, either alone, or mixed with 1, 2, 3, 4, up to 12 volumes of air, oxygen, or hydrogen, by from half a volume to a volume of sulphuric acid, it was entirely absorbed and removed.

When olefiant gas is present, additional care is required in analytical experiments, in consequence of the gradual combination of the olefiant gas with the sulphuric acid. I found that 1 volume of sulphuric acid in abundance of olefiant gas, absorbed about 7 volumes in twenty-four hours in the dull light of a room; sunshine seemed to increase the action a little. When the olefiant gas was diluted with air or hydrogen, the quantity absorbed in a given time was much diminished; and in those cases it was hardly appreciable in two hours, a length of time which appears to be quite sufficient for the removal of any of the peculiar vapours from oil or coal gas.

My mode of operating was generally in glass tubes over clean mercury*, introducing the gas, vapour or mixture, and then throwing up the sulphuric acid by means of a bent tube with a bulb blown in it, passing the acid through the mercury by the force of the mouth. The following results are given as illustrations of the process:

Oil Gas from a Gasometer.

	sul. acid		in 8′	in 1 hour	2 hours	dimin. per cent.
188 vol.	+ 9·5	vol. diminished to	155·0	148·5	146·4	22·12
107	+ 13·0	.....	88·5	84·5	82·0	23·33
138	+ 5·2	.....	113·7	108·0	106·5	22·82

* If the mercury contain oxidizable metals, the sulphuric acid acts upon it, and evolves sulphurous acid gas. It may be cleaned sufficiently by being left in contact with sulphuric acid for twenty-four hours, agitating it frequently at intervals.

Oil

Oil Gas from Gordon's Lamp.

	sul. acid		15′	30′	3 hours.	dimin. per cent.
214 vol.	+	6·8 vol. diminished to	183·3	180·8	176·0	17·75
159	+	5·9	137·5	136·0	130·4	17·98
113	+	12·2	98·0	96·0	92·0	18·58

Coal Gas of poor Quality.

548·6	+	27·6	533·3	529·2	529·0	3·57
273·6	+	27·8	267·9	266·0	266·0	2·78
190·6	+	13·1	186·0	184·2	184·1	3·41

Oil may also be used in a similar manner for the separation of these vapours. It condenses about 6 volumes of the most elastic vapour at common temperatures, and it dissolves with greater facility the vapour of those liquids requiring higher temperatures for their ebullition. I found that in mixtures made with air or oxygen for detonation, I could readily separate the vapour by means of olive oil; and when olefiant and other gases were present, its solvent power over them was prevented by first agitating the oil with olefiant gas or with a portion of the gas to saturate it, and then using it for the removal of the vapours.

In the same way some of the more fixed essential oils may be used, as *dry* oil of turpentine; and even a portion of the condensed liquor itself, as that part which requires a temperature of 220° or 230° for its ebullition; care being taken to estimate the expansion of the gas by the vapour of the liquid, which may readily be done by a known portion of common *air preserved over* the liquid as a standard.

With reference to the proportions of the different substances in the liquid as obtained by condensation of oil gas, it is extremely difficult to obtain any thing like precise results, in consequence of the immense number of rectifications required to separate the more volatile from the less volatile portions; but the following table will furnish an approximation. It contains the loss of 100 parts by weight of the original fluid by evaporation in a flask for every 10° in elevation of temperature, the substance being retained in a state of ebullition.

100 parts at 58°	parts.	differences.
had lost at 70 . . .	1·1	
		1·9
80 . . .	3·0	
		2·2
90 . . .	5·2	
		2·5
100 . . .	7·7	
		2·4
110 . . .	10·1	
		3·1
120 . . .	13·2	
		2·9
130 . . .	16·1	
		3·2
140 . . .	19·3	
		3·1

150° . . .	22·4	
		3·2
160 . . .	25·6	
		3·4
170 . . .	29·0	
		15·7
180 . . .	44·7	
		23·4
190 . . .	68·1	
		16·1
200 . . .	84·2	
		7·4
210 . . .	91·6	
		3·7
220 . . .	95·3	
		1·3
230 . . .	96·6	

The residue, 3·4 parts, was dissipated before 250° with slight decomposition. The third column expresses the quantity volatilized between each 10°, and indicates the existence of what has been described as bi-carburet of hydrogen in considerable quantity.

The importance of these vapours in oil gas, as contributing to its very high illuminating powers, will be appreciated, when it is considered that with many of them, and those of the denser kind, it is quite saturated. On distilling a portion of liquid, which had condensed in the pipes leading to an oil gas gasometer, and given to me by Mr. Hennel, of the Apothecaries' Hall, I found it to contain portions of the bi-carburet of hydrogen. It was detected by submitting the small quantity of liquid which distilled over before 190° to a cold of 0°, when the substance crystallized from the solution. It is evident therefore, that the gas from which it was deposited must have been saturated with it. On distilling a portion of recent coal gas tar,—as was expected, none could be detected in it; but the action of sulphuric acid is sufficient to show the existence of some of these bodies in the coal gas itself.

With respect to the probable uses of the fluid from compressed oil gas, it is evident in the first place, that being thus volatile, it will, if introduced into gas which burns with a pale flame, give such quantity of vapour as to make it brightly illuminating; and even the vapour of those portions which require temperatures of 170°, 180°, or higher, for their ebullition, is so dense as to be fully sufficient for this purpose in small quantities. A taper was burnt out in a jar of common air over water; a portion of fluid boiling at 190° was thrown up into it, and agitated: the mixture then burnt from a large aperture with the bright flame and appearance of oil gas, though of course many times the quantity that would have been required of oil gas for the same light was consumed; at the same time there was no mixture of blueness with the flame, whether it were large or small. Mr. Gordon has, I understand, proposed using it in this manner.

The

The fluid is also an excellent solvent of caoutchouc, surpassing every other substance in this quality. It has already been applied to this purpose.

It will answer all the purposes to which the essential oils are applied as solvents,—as in varnishes, &c.; and in some cases where volatility is required, when rectified it will far surpass them.

It is possible that, at some future time, when we better understand the minute changes which take place during the decomposition of oil, fat, and other substances by heat, and have more command of the process, that this substance, among others, may furnish the fuel for a lamp, which remaining a fluid at the pressure of two or three atmospheres, but becoming a vapour at less pressure, shall possess all the advantages of a gas lamp, without involving the necessity of high pressure.

Royal Institution, June 7, 1825.

[In relation to the subject of this paper, see Phil. Mag. vol. lvi. p. 455, and lvii. p. 3.—Edit.]

XXVII. *On some Kinds of Fulminating Powder inflammable by Percussion, and their Use in Fire-arms.* By P. W. Schmidt, *Lieutenant in the Prussian Service.**

A POWDER inflammable by percussion has been used for some years past, especially in fowling-pieces. The following formulæ have been given for the preparation of this powder, the principal ingredient of which is chlorate of potash.

1.) 100 parts of chlorate of potash (fulminating salt), 12 parts of sulphur, and 10 parts of charcoal are closely mixed. The grains are produced by forcing the soft paste through a sieve.

2.) 100 parts of chlorate of potash, 42 parts of saltpetre, 36 parts of sulphur, and 14 parts of lycopodium.

These are the usual ingredients that have hitherto been mixed with the chlorate of potash for the purpose of making priming powder. The guns, however, with which this powder is used, are very various in their construction. In some it explodes of itself by means of the mechanism of the lock, on being cocked into a small conical recess, which communicates with the touch-hole; in others, it is put in previous to every shot. In the former kind of guns a quantity of powder sufficient for a certain number of shots is kept in a recess attached to the lock, called a magazine; and the locks (which were invented in England by Mr. Forsyth) are called magazine-locks.

* From Schweigger's *Journal*, Band xi. p. 66.

In

In some guns the stroke of the cock, which is in the shape of a hammer, falls immediately on the fulminating powder strewed in the above recess. In order to protect the powder from wet, small balls of it were covered over with wax, and placed sometimes in the conical recess, and at others fixed to the cock itself. In both instances the ball was kindled in the recess just mentioned, by means of the percussion.

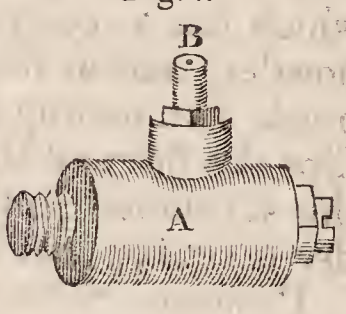
Besides these, other contrivances have been used for the purpose of igniting this kind of powder; yet they have all their defects, and offer so many difficulties in practice as to have prevented their general introduction.

Latterly they have contrived in Germany to fix the powder in a small case of very thin copper foil (fig. 1), for the purpose of keeping it dry; and for that purpose made the gun as shown in fig. 2: viz. the cylinder A is screwed into the body of the gun instead of the touch-hole, and rests for the sake of greater support on the plate of the lock, instead of resting on the pan. The inner space of the cylinder is filled in loading with the same powder as that of the shot. The igniting-box, at the bottom of which is the detonating powder, is, previous to firing, upturned on the cylinder B. In this cylinder is a small round aperture leading to the inner space of the cylinder A. On the trigger being pulled, the cock strikes the igniting-box, and the fulminating powder is kindled by the blow, flows through the aperture, inflames the shot, and breaks the igniting-box.

Fig. 1.



Fig. 2



Mr. Wright seems to have taken great pains with the subject*. He recommends, for the firing-cases, to use fulminating mercury, saying that sportsmen had justly complained of the powder made of chlorate of potash, since it soon produces the oxidation of the barrel and touch-hole, and the charcoal which remains after the firing rendering them unfit for use. The advantages of his new powder he enumerates as follows:—It does not make the gun rusty so soon as the other; it produces neither dirt nor moisture; it is not so liable to explode as the other powder, and if it does explode, its effects are less destructive, inasmuch as its power does not extend so far.

The following is his mode of preparation:

"I place two drachms of quicksilver in a Florence flask,

* Mr. Wright's paper will be found in Phil. Mag. vol. lxii. p. 203.

and

and pour six drachms (measure) of *pure* nitric acid on the mercury: this I place in a stand over a spirit-lamp, and make it boil, till the quicksilver is taken up by the acid;—when nearly cool, I pour it on an ounce (measure) of alcohol in another flask: sometimes *immediate* effervescence ensues, with the extrication of nitrous æther; and often I have been obliged to place the mixture over the lamp, till a white fume begins to rise, when the effervescence follows. I suffer the process to continue (removing the lamp) till the fumes assume a reddish hue: when I pour water into the flask, and the powder is found precipitated to the bottom, I pour off, and add fresh water, permitting the powder to subside each time before the water is poured off, so as to free the substance as much as possible from the acid; and then I pour it on a piece of filtering paper, and place the powder in an airy room to dry. It should be kept in a corked (not stoppered) bottle."

For the filling of the caps he makes use of an ivory pin, which has a scoop at one end for the purpose of receiving the powder, and at the other is cut flat. With it he puts in as much fulminating mercury as will cover the bottom: he then dips the flat end into a strong tincture of benzoin, and rubs this substance gently about the case; by which means the powder is set fast and covered as with a varnish.

Professor Schweigger, speaking of these kinds of experiments in his chemical lectures, observed on the practicability of kindling gunpowder by the admixture of other substances, as has been shown in a criminal investigation that took place at Munich a few years ago.

A box filled with gunpowder was sent to an individual provided with fulminating papers, which were to inflame on the box being opened. Fortunately, however, the murderous design was frustrated; for although the papers exploded, they did not kindle the powder. The assassin was discovered and punished. M. Gehlen, who had been examined at the trial, was led by the circumstance to make several experiments for the purpose of kindling gunpowder by means of Brugnatelli's fulminating silver, but they all failed.

It seems that in England, too, difficulties had been found in igniting gunpowder with fulminating mercury; for Mr. Wright observes, " If any one doubts the practicability of firing gunpowder by means of fulminating mercury,—by procuring a percussion gun he may try the experiment and be fully satisfied."

Professor Schweigger having therefore requested me to try some experiments on this subject, especially with fulminating silver, I made them in the chemical laboratory of our university, and the following were the results.

I.) Ful-

I.) Fulminating silver was prepared in the usual way; five drachms of fuming nitric acid and five of alcohol were poured over one drachm of fused nitrate of silver. When the effervescence and solution were complete, water was added. The precipitate of fulminating silver was filtered off, and all the remaining acidity washed from it with water. The liquid which had passed through the filter gave with muriatic acid a copious precipitate of chloride of silver. The fulminating silver, which was of a whitish tint, was now subjected to the following experiments:

1.) When damp it ignited very rarely, and only by a strong blow. When dry, it explodes easily, and with a much slighter blow.

2.) When touched with sulphuric acid, it exploded equally strongly, whether damp or dry.

3.) Damp or dry, it exploded in the fire.

4.) The substance which remained after the ignition was of a blueish brilliant hue and a disagreeable metallic taste. I could gather but little, which, dissolved in water, produced a faint red tint on litmus paper.

5.) I failed in several attempts to ignite gunpowder with the fulminating silver. I therefore put quantities, of the size of a small pin's head, into some copper boxes, fastening it in some with tincture of benzoin, in others with a solution of gum arabic in water; and others I tried to press on the bottom without any other aid. I applied them to guns prepared for the use of igniting-boxes, and thus kindled the gunpowder with incredible swiftness. The series of experiments thus made in the presence of Professor Schweigger, leave no doubt that fulminating silver will easily ignite gunpowder in a manner that will secure it against a rapid dispersion on exploding.

II.) The fulminating mercury was prepared in the manner prescribed by Mr. Wright. But I must observe that the experiment only succeeded by the application of fuming nitric acid. The fulminating mercury, when obtained, was washed till every particle of acidity had disappeared from it. It was then submitted to the following experiments:

1.) When dry, it exploded like fulminating silver, and with a much slighter blow than is required for the powder made of chlorate of potash. Thus it would appear as if the fulminating quicksilver had not in this respect the advantage over the igniting powder made with the salt just named.

2.) When perfectly dry only it could be ignited with sulphuric acid.

3.) In the fire it exploded, both wet and dry.

4). The

4). The substance remaining after the explosion, had a blueish brilliant tint and a bitter acid metallic taste. With a small quantity dissolved in water, litmus paper was slightly reddened. I made no further investigations; inasmuch as the examination of the acids of fulminating metals, before and after the explosion, was not my object; especially since Dr. Liebig has lately published a series of very interesting experiments, the repetition of which would require very extensive labour *. Liebig calls those acids fulminic acids, which, being the property of all metals, he distinguishes into silver- and mercury-fulminic acids, &c.

The great advantages, however, of the fulminating quicksilver as igniting powder, extolled by Mr. Wright, I did not find confirmed; although I proceeded to fill copper boxes, as I had done with the detonating silver, which all ignited the charge.

III.) I also submitted to experiment the first-named mixture, principally consisting of chlorate of potash, and found,

1.) That it exploded only by a hard blow. Its effects were much less than those of the detonating silver or quicksilver. Mr. Wright, indeed, says the contrary of the latter: it seems therefore that I used a better kind of fulminating mercury;

* Vide *Ann. de Chim. et de Phys.* part xxiv. p. 294, or the translation in Gilbert's *Annalen der Phys.* part lxxv. p. 393—422. Mr. Liebig found that Brugnatelli's detonating silver was dissolved in lime-water or solutions of the caustic alkalies, whereby 31·25 per cent of oxide of silver was deposited. They produce peculiar salts called *fulminates*, which explode with great violence. These salts are dissolved by nitric acid, sulphuric acid, and acetic acid: the silver fulminic acid contained in them, and so difficult to be dissolved, is deposited; and by heating a solution of fulminate of lime to the boiling point, and adding a moderate quantity of nitric acid, is deposited, on cooling, at the bottom of the vessel, in the shape of long white crystals. This acid may be easily dissolved in boiling water; from which it crystallizes again in cooling, has a disgusting metallic taste, and reddens litmus paper. But it cannot subsist of itself without combination with a metal; and in the same manner as there are prussic acids of iron, copper, silver, and gold, so the fulminic acid combines with silver, quicksilver, copper, iron, zinc, &c., into proper fulminic acids, which again form different combinations with the bases, *e. g.* potash, soda, barytes, strontian, lime, &c. Thus, for instance, silver-fulminate of potash consists of 35·03 parts of silver-fulminic acid and 14·92 of potash; silver-fulminate of soda, of 88·66 parts of silver-fulminic acid and 11·34 of alkali. When cooling, Berthollet's detonating silver forms granular shining white crystals. One part of this salt makes as violent a report as three parts of Howard's (Brugnatelli's) fulminating silver. With magnesia the silver-fulminic acid combines in two ways. One combination is a simply decrepitating, not detonating, insoluble powder, of a rosy tint: the other forms white capillary crystals, and explodes very loudly. The first combination was used for the analysis of fulminic acid in the dry way; in which the fulminating silver was evinced to consist of 32·22 of oxygen, 3·22 of hydrogen, 11·28 of azote, 9·68 of carbon, and 41 of silver.

but for that very reason I must give the chlorate of potash the preference for practical use.

2.) In the fire it puffed away like gunpowder.

3.) The substance remaining after the explosion is blackish and dusty, and contains less of acidity than that left by the quicksilver. Thus, and indeed from all my experiments, it is evident that it oxidizes the iron less than the fulminating mercury. Moisture is also left by the latter; and the charcoal left by the mixture of chlorate of potash, after kindling a copper box filled with it, is very unimportant:—therefore this mixture is preferable as an igniting powder. This is also the reason why the manufacturers no longer use the fulminating quicksilver. I know one who makes and fills weekly several thousands of copper caps, for which he uses the chlorate mixture, the preparation of which is both less expensive and less dangerous than that of the fulminating quicksilver. There is another circumstance attending this mixture. In filling the caps, it will sometimes happen that the quantity put in is doubled, which I find is of no injurious consequence with this mixture; but might endanger the person firing with the fulminating mercury, as the box will burst too violently.

With respect to the power of igniting the charge, the different kinds of powder which I have compared are equally effectual.

IV.) I submitted the mixture of chlorate of potash mentioned above to the following experiments.

1.) That part only ignited which was struck, without igniting that lying around it.

2.) In the fire it burns away with noise.

3.) I placed it in the usual way in copper caps, but could not ignite a charge with them. The cause of this may be explained by the construction of the locks, with reference to the properties of this detonating powder. That part of the cap situated just above the opening of the cylinder B remains, as the blow cannot fall on it unignited, as shown by the experiment No. 1. But the communication of the ignited part with the charge, is prevented by the manner in which the cock strikes the cap. In guns in which such powder is used for igniting, it lies, as above stated, in small balls in a conical aperture. Here it is nearly all ignited by the striking of the cock, and must of necessity flow inwards, every other way of escape being shut up.

In conclusion:—I have to add that the method of filling the caps recommended by Mr. Wright is not only laborious, but even dangerous. How are manufacturers to employ that method when they have to fill several thousands a week? I have

made

made various trials, and the following process seems to me to be the best.

Pour some adhesive solution or tincture over the powder, and mix it with it into a stiff kind of liquid. Take with a brush or a stick a large drop of it, and apply it against the bottom of the cap.

This method is both quick and free from danger; whilst on filling with the dry detonating powder, the least careless touch may produce an explosion.

In order to prevent the corrosion of the cylinder, and its becoming useless by the formation of sulphuret of iron (an evil very common with iron touch-holes, and caused more by the action of the gun-powder than by that of the igniting substance), the inside of the cylinder should be lined with a metal which will neither oxidate, nor easily combine with the ingredients of the powder.

XXVIII. *On Oil Gas*. By* M. Juste Preuss, *resident in Paris, formerly Inspector of Rivers and Forests of the* Domaine Extraordinaire *in Germany, and Member of several learned Societies.*

THE prevalence of lighting with coal gas had gone on with so much rapidity in England before Messrs. John and Philip Taylor of London had conceived the happy idea of making gas by means of oil, that after they had carried this branch of industry to a certain degree of perfection, the quarters of the metropolis and the provincial towns, where lighting with gas would have presented advantages, were already occupied by coal gas. The progress of the use of oil gas has necessarily been slow: it was first adopted in dwelling-houses and in gentlemen's seats, where they soon learned to esteem it on account of its salubrity and the beauty of its light, and because it does not change the gildings, plate, and stuffs, nor the paint nor pictures, as coal gas often does. Afterwards it was adopted in manufactories; still later in small towns and on high roads; and at present it is chosen in preference, even in the provinces which abound in coal pits, for the lighting of towns of the first order, to the great chagrin, and notwithstanding the opposition and clamours, of the coal

* Extracted from the *Messager des Sciences et des Arts,* Decembre 1824. This scientific selection is published by the Royal Societies of the Fine Arts and of Agriculture at Ghent. It forms annually one volume of thirty sheets, in 8vo., embellished with several engravings, and is published in numbers, of from two to eight sheets, at Ghent, by P. F. De Goesin-Verhaeghe, No. 37, Hautport-street, at fifteen francs to subscribers.

gas companies united, or, so to speak, leagued together, against this too dangerous competitor.

Oil gas possesses some characteristic properties which have procured it this preference which is given to it in England over coal gas. These properties not being generally known, I shall state some of the most remarkable. One single cubic foot of oil gas gives as much light as $3\frac{1}{2}$ cubic feet of coal gas *. By this important fact the capacity of the gasometers (the most inconvenient part of the former system of lighting) is now reduced to less than a third part; the pipes to one third of their former capacity; the size of the retorts, stoves, and apparatus for purifying and condensing, to one sixth; the cost of the apparatus generally, and the expense of keeping them up, to one third; lastly, the labour to less than the eighth part of that which it costs in a large establishment to feed with coal gas an equal number of burners of a given intensity. It will perhaps be objected to me, that the first material, the oil, on the one hand, is a dear and valuable commodity, whilst, on the other, coal is much less so in proportion. I grant it as regards certain countries; but I will add, that the daily expenses of a gas establishment are of two natures,—either permanent, or susceptible of variation according as the sale of the gas diminishes or augments: the first are principally composed of the interest on the capital employed, of the rent of premises, and of the labour, as the labourers cannot be dismissed during the summer months, who have been instructed at an expense during the winter. Now these permanent expenses are inconsiderable in an oil gas work, and, comparatively, very great in a coal gas establishment. The second class of expenses includes the raw material, the consumption of which follows in exact proportion the sale of gas; and on this head gas works of each kind enjoy the same advantage. The winter season, which brings with it long evenings and revives balls, concerts, masonic meetings, &c., augments and often increases tenfold *the demand for light*. A gas establishment ought to be prepared to meet those often unexpected calls, all of which decrease, and partly disappear with spring. Here then is what happens to the two kinds of lighting establishments, as Dr. Ricardo has judiciously observed before me: it is, that in the coal gas work the great expense remains the same, and the lesser expense diminishes; whilst in the manufactory of oil gas the lesser expense continues, and the great expense decreases, or even ceases, in equal proportion with the sale of light.

The inconvenient heat diffused by the combustion of coal

* Each being of the mean quality, that is to say, specific gravity: as for example the coal gas 0·4069, and the oil gas 0·9395, the air being 1·0000.

gas

gas in shops, &c., is often complained of, and which, in effect, is proportioned to the number of cubic feet which are hourly burnt. It is almost superfluous to observe here that oil gas, for equal light, gives no more heat than Argand lamps. To all these advantages, oil gas also unites that of being the only one which is suited for compression in portable lamps and in reservoirs, on account of its richness in light under a little volume.

As light only is wanted, we are not forced, as with coal gas, to create at the same time disagreeable residues, and accessory products, which would be gladly dispensed with; but on the more or less advantageous sale of which depends, nevertheless, the profit or loss of the undertaking; in short, in distilling oil, gas only is obtained.

The rise or fall of oils is moreover nearly indifferent to the manufacturer who knows how to produce good gas with raw oils of the most inferior qualities, such as will cost him, for example, forty-seven francs the hectolitre; whilst the consumer, to supply Argand lamps, must buy purified oil, perhaps at the rate of fifty-seven francs; besides that the light of an oil lamp is necessarily influenced by a number of circumstances more or less favourable: as by the length and uniform height of the wick; by the fineness of the fibres of the cotton of which it is composed, and which help the capillary attraction; by its dryness, for it is a body sensibly hygrometric, and if it has become charged with humidity from the air, it is thereby less fit to imbibe the oil; by its state of carbonization more or less advanced; by the actual level of the oil, always lowering (except in the beautiful lamp of Carcel); by the quality of the oil itself, and by the processes of purification it has undergone; lastly, by the more or less care with which the Argand lamp has been daily cleaned, &c.; not to speak of the form of glasses and other circumstances which modify generally the effect of the various kinds of lights, without excepting gas.

Although these details may appear very trifling, they are notwithstanding, without exception, of such importance, that if there is a disproportion in one single point, the light of the Argand lamp must necessarily be imperfect; it is seen for this same reason how much this mode of lighting is subject to chance. I will say more: it is on the coincidence of all these nice points, with a just proportion of dry ambient air flowing in with a constant and regular speed, that the temperature depends at which the decomposition of the oil and the consequent combustion of the gas is effected. In short, if the temperature is too low, a portion of oil escapes, either under the form of oily vapour, or under that of smoke, without being burnt: if on

the

the contrary the temperature is too high, the combustion is indeed complete and without smoke, but there is a partial destruction of light. (This last point is the most delicate in the art of lighting, since the phænomenon is not directly perceptible by our senses.) In each case, a variable proportion of oil is expended in pure loss. Accordingly, I believe I do not exaggerate in stating, that Argand lamps which give the maximum of light due to the weight of oil they consume, are not less rare than capital prizes in the lottery.

Lighting with gas, on the contrary, is a process comparatively very simple, which dispenses with all care and attention; for the gas which freely escapes under a regular pressure through the orifices of a burner well executed and judiciously proportioned in its dimensions, cannot fail to give an invariable maximum of light.

It is by these considerations only that I have been able to account to myself for a fact which appears very extraordinary at first sight; namely, that 100 pounds of raw oil converted into gas in a large apparatus, such as are constructed by Messrs. Taylor and Martineau of London for the lighting of towns, produce a quantity of light which cannot be produced by Argand lamps without burning at least 130 pounds, sometimes 150 pounds of purified oil; and it happened to my friend the learned Professor M. Clement Desormes and myself, to make experiments in London on a quite new Paris Argand lamp, which consumed in the proportion of 318 pounds of oil for 100 pounds which its light only represented, in comparison with oil gas.

September, 1825.

XXIX. *On the Comparative Advantages of Oil and Coal Gas. By* Robert Christison, *M.D. F.R.S.E., Professor of Medical Jurisprudence; and* Edw. Turner, *M.D. F.R.S.E. Lecturer on Chemistry, Edinburgh.**

THE paper (read before the Royal Society of Edinburgh, April 18, and May 2, 1825) from which the following extract is given, contains full details of a series of experiments undertaken at Edinburgh, as subordinate, in the first instance, to an inquiry regarding the illuminating power of oil and coal gases, at a time when, from the projected establishment of an oil gas company, the question of the illuminating power of the gases excited an extraordinary interest in that city; and the subject being taken up under the powerful influence of private interest, a variety of statements were published by several scientific gentlemen as the result of their experiments, which, in-

* From the Edinburgh Philosophical Journal, vol. xiii. p. 34.

stead

stead of rendering the matter clearer, and receiving the confidence of men of science and of the public, differed so widely from what had been previously obtained in London and elsewhere, that a necessity was generally felt for further and more varied experiments, before a question in which such an immense capital was involved throughout the kingdom, could be held as definitively settled.

The experiments relate, 1st, To the instruments employed in the inquiry, and particularly the photometers of Leslie and Rumford; that of the latter was preferred, Mr. Leslie's being found inapplicable, on account of its being affected by non-luminous heat, and not expressing accurately the power of lights differing in colour. 2ndly, To the circumstances which affect the degree of light emitted by the gases during combustion, and which lead to the discovery of the principles on which burners ought to be constructed:—these circumstances are arranged under three heads, the length of the flame, the construction of the burner, and the shape of the glass chimney. 3rdly, The relative Illuminating power of Oil and Coal Gas.

The authors conclude the account of their experiments* by stating that it was not originally their intention to make any remarks on the relative advantages of the two gases in a general point of view. But as the subject has lately led to a long parliamentary investigation, and as very erroneous notions prevail on some matters which have engaged a share of their attention, they add that it may be well to notice it briefly.

The question of the relative advantages of oil and coal gas resolves itself into two: The first regards their relative œconomy; The second, their comparative utility.

1. Before we can determine their relative œconomy, it is requisite to settle their average quality. Taking their specific gravity as the ground of comparison, we apprehend that, in small towns, where the cannel coal can be had at a low price, coal gas companies may be able to manufacture a gas of the density of 700. In larger cities, such as Glasgow and Edinburgh, where coal of every kind is dearer, and the cannel coal cannot easily be procured in sufficient quantity, the average

* While the philosophers of Edinburgh were engaged in these interesting inquiries, a celebrated Committee in Westminster have had to decide on the same subject. Here, the most practical and conclusive experiments were not, as in the North, on the illuminating power of the gases, but on the power of motives on divers members of the committee. Thus, by a judicious distribution of their shares to some, by making others Directors with large salaries, and by lighting gratuitously the mansions of others, the powerful combination of coal gas companies obtained a decision, not indeed that their gas was at all equal to oil gas, but that, in the metropolis at least, the latter should not be allowed to come in competition with it.

specific

specific gravity of the gas will not exceed 600. And in such a town as London, where the cannel coal can scarcely be procured at all, the average specific gravity will not exceed 450.

The average specific gravity of oil gas should eventually be the same every where. It is difficult to ascertain what the average is at present, as made by large establishments; but there is no substantial cause why it should fall short of 920. We have assigned strong reasons, however, for believing that it must be soon improved considerably. This improvement indeed may be no great gain; for the question will then occur, whether it can be effected without diminishing the quantity of gas in the same proportion with its increase in quality. It is generally supposed that an improvement in the quality of oil gas is necessarily attended by a loss in quantity; but, so far as can be discovered, this idea rests on experiments performed by operatives only, whose authority we are satisfied, from repeated observation, can by no means be relied on. If charcoal is left in the retorts at the end of each charge, it is clear that the gas may be improved by the addition of all this charcoal, without any diminution in quantity; for if it be added to the light carburetted hydrogen, which gives little light, so as to convert it into the olefiant gas, which is powerfully illuminating, the change, it is well known, will take place without any alteration in volume. On the other hand, if good oil gas be exposed to a high temperature, it is partly decomposed, and deposits some of its charcoal. Part of the olefiant gas becomes light carburetted hydrogen, and without any increase in volume; for the volume is not increased unless it is resolved into charcoal and hydrogen. Hence a bad gas may be made from oil, which shall not exceed in quantity the good gas of Taylor and Martineau. And in point of fact, we have several times found, when the retorts were choked with charcoal, and the specific gravity of the gas was only 660, that the quantity fell short of 100 cubic feet per gallon, which is said to be about the average produce when the gas is good. When oil gas has a specific gravity of 910, charcoal is still found in the retorts. It may therefore be improved by the addition of all this charcoal, and still retain its volume. Besides, it may be possible to improve it by the addition of charcoal from other sources. Hence, while we at present assign to oil gas the average specific gravity of 920, we cannot help anticipating a considerable improvement and positive gain.

From what has been said of the average quality of coal gas in different quarters of the kingdom, it is clear that the question of its œconomy, compared with oil gas, can be only answered relatively. In Edinburgh and Glasgow, where coal is moderately cheap, and coal gas of good quality, oil gas must be

be somewhat dearer; in London, where the coal is dear, and the gas bad, oil gas should be positively cheaper; and in other places the two will be nearly the same in price. This statement is, of course, drawn from our own experiments on their illuminating power, coupled with the well-known computations of Accum, Peckston, Ricardo, and others, regarding their relative cost.

The second element in the question of their relative advantages, is their comparative utility. It is certain that whatever difference may exist between them in this respect must be in favour of oil gas.

In the first place, the quality of the light is superior. It is whiter, and has a peculiar sparkling appearance, superior to that of coal gas. It is therefore a more beautiful light, fitter for the artificial illumination of colours, and not liable to give the human countenance that unpleasant sallow appearance which every one has observed to be caused by coal gas.

An objection has been urged to the employment of gas in general, that it has a disagreeable odour. This objection does not apply at all, unless the gas is unconsumed; for neither oil nor even coal gas, so far at least as our observation goes, emits any odour if properly burnt. But if they escape and mix with the air, their presence is then readily detected by the smell. The odour of oil gas is purely empyreumatic, but quite distinct: we have possessed occasional specimens, which had a faint smell, but we never found it altogether inodorous. The best oil gas appears to have the least smell. The odour of coal gas is of a mixed kind, being in part empyreumatic like oil gas, and partly of an exceedingly offensive nature, like that of sulphuretted hydrogen. In Edinburgh coal gas we have generally observed the empyreuma alone; but frequently the other is perceptible also, and sometimes it prevails to an insufferable degree.

The most serious objection to coal gas arises from the presence of impurities. These are, a black matter like tar, and compounds of sulphur,—all derived from the coal itself, and therefore necessarily present originally in every description of coal gas. Without purification, therefore, coal gas could scarcely be used at all; and it becomes a question of importance to determine whether or not the noxious ingredients may be wholly removed from it. The greater part of the tar is deposited at the works in the proper vessels, but a minute portion does commonly pass over with the gas. It tends to clog the apertures of the burner, and of course soils substances upon which it is deposited. In common shops, where a free current of air is preserved, the effect is hardly noticed; but

we suspect that a part of the inconvenience found by jewellers to attend the use of coal gas arises from this cause.

The most formidable of the compounds of sulphur present in coal gas is sulphuretted hydrogen. The presence of this gas is hurtful in two ways. If it escape unburnt, it offends by its insupportable odour, and attacks silver and paint with great readiness. When consumed, it forms sulphurous and sulphuric acids, which may injure the health if habitually inspired, and act chemically on various substances, as on iron and steel. Hence the necessity of removing it entirely from coal gas. On this subject two important questions naturally occur, to both of which we can give a decisive answer. 1st, Can sulphuretted hydrogen be wholly separated from coal gas? and 2dly, when it is removed, Can coal gas be regarded as perfectly free of sulphur?

We are satisfied that sulphuretted hydrogen may be wholly removed; for we have repeatedly examined the Edinburgh coal gas by the most delicate tests, without detecting a trace of it. Of course we do not vouch that it is always equally pure, because the least neglect on the part of the workmen must inevitably cause some sulphuretted hydrogen to escape into the pipes. It is equally certain, however, that coal gas, when completely free of sulphuretted hydrogen, still contains sulphur. On burning a small jet of coal gas, free from sulphuretted hydrogen, so as to collect the fluid formed during the combustion, the presence of sulphuric acid was uniformly detected, demonstrating the existence of some compound of sulphur. What that compound is has not yet been ascertained; but from its peculiar unpleasant odour, and the circumstances under which it is generated, the sulphur is most probably in combination with carbon, either in the form of the volatile liquid (sulphuret of carbon, as Mr. Brande conjectures), or, what is perhaps more likely, as a gaseous compound, containing a less proportion of sulphur than exists in that liquid.

In whatever state of combination the sulphur may be, it does not affect the salts of lead like sulphuretted hydrogen; nor does it act so readily, if at all, on polished silver and gold. Hence the gas which contains only this impurity will be less injurious when any of it escapes unburnt, than such as contains sulphuretted hydrogen; but since it uniformly yields acid vapours during its combustion, one part of the objection remains in full force.

These various objections, whatever weight they may have, apply to coal gas only.

XXX. *A short Method of finding the Latitude at Sea by Double Altitudes and the Time between.* *By* James Burns, *B.A.*

THIS problem, from its great utility to seamen, has engaged the attention of several eminent mathematicians: but among the many attempts that have been made, not one short *direct* and *accurate* method has been yet given. The solution given by Douwes was the only one generally practised, for a long time; but there are many objections to this method, which the scientific navigator will easily comprehend. In the first place, its being a method of *false position*, and depending chiefly on the latitude by account, renders it in most cases inaccurate; and moreover, to ensure any thing like correctness, the computed latitude should be nearer the true latitude than that by account. Secondly, its necessary limitations, with respect to the time, must often render it impracticable.—Another method was given a few years ago by Dr. Brinkley, in the Nautical Almanac; but it has been found so tedious, that very few seamen, I believe, have ever practised it. Besides, it is liable to the principal objections that are made to Douwes's solution; for the latitude by account is retained in it, and two or three repetitions and corrections of the calculation are necessary, before any conclusion to be depended on can be had: and after all, a considerable degree of error might be involved in the result. In the following investigation,—which is *direct*,—the consideration of the latitude by account is therefore omitted. All that is necessary to be known is, the time, the interval between the observations, and the altitudes; all of which, from the improved state of our chronometers and other instruments, may be known with the greatest exactness.

Let A = greater altitude,
a = less altitude,
δ = $\odot$'s declination,
λ = the latitude,
τ = the time *from* or *to* noon corresponding to A,
ι = the interval.

By elementary spherical trigonometry, we have,

$$\begin{aligned}
\sin. A &= \cos.(\lambda \pm \delta) - \text{vers.}\,\tau . \cos.\delta . \cos.\lambda . \\
&= \cos.\lambda . \cos.\delta \mp \sin.\lambda . \sin.\delta - \{(1 - \cos.\tau) . \cos.\delta . \\
&\qquad \cos.\lambda . \} . \\
&= \cos.\lambda . \cos.\delta \mp \sin.\lambda . \sin.\delta - (\cos.\delta . \cos.\lambda - \cos.\delta . \\
&\qquad \cos.\lambda . \cos.\tau) . \\
&= \cos.\lambda . \cos.\delta \mp \sin.\lambda . \sin.\delta - \cos.\delta . \cos.\lambda + \cos.\delta . \\
&\qquad \cos.\lambda . \cos.\tau . \\
&= \mp \sin.\lambda . \sin.\delta + \cos.\delta . \cos.\lambda . \cos.\tau .
\end{aligned}$$

By a similar investigation we should find,

$$\sin.\ a = \mp \sin.\ \lambda . \sin.\ \delta + \cos.\ \delta . \cos.\ \lambda . \cos.\ (\tau + \iota).$$

$\therefore$ by subtraction,

$$\sin.\ A - \sin.\ a = \cos.\ \delta . \cos.\ \lambda . \cos.\ \tau - \cos.\ D . \cos.\ \lambda . \cos.\ (\tau + \iota).$$

$$= \cos.\ \lambda . \{\cos.\ \delta . \cos.\ \tau - \cos.\ \delta . \cos.\ (\tau + \iota)\}.$$

$\therefore \cos.\ \lambda =$

$$\frac{\sin.\ A - \sin.\ a}{\cos.\ \delta . \cos.\ \tau - \cos.\ \delta . \cos.\ (\tau + \iota)} = \frac{2 \cos.\ \frac{1}{2}(A + a) . \sin.\ \frac{1}{2}(A - a)}{\cos.\ \delta . \{\cos.\ \tau - \cos.\ (\tau + \iota)\}} =$$

$$\frac{2 \cos.\ \frac{1}{2}(A + a) \sin.\ \frac{1}{2}(A - a)}{2 \sin.\ \left(\frac{2\tau + \iota}{2}\right) . \sin.\ \frac{1}{2}\iota . \cos.\ \delta.} = \frac{\cos.\ \frac{1}{2}(A + a) . \sin.\ \frac{1}{2}(A - a)}{\sin.\ (\tau + \frac{1}{2}\iota) . \sin.\ \frac{1}{2}\iota . \cos.\ \delta.} \ldots (1)$$

Such is the formula of calculation, when the observations are made on the *same* side of noon; and the practical rule may be expressed as follows: Add together the log. cos. of half the sum, and the log. sin. of half the difference of the two altitudes; from the sum of these two logarithms, increased by 20 in the index, subtract the log. cos. of the declination, the log. sin. of the time, and half interval reduced to degrees, and log. sin. of half the interval;—the remainder will be the log. cos. of the *true* latitude.

When the observations are made on *different* sides of noon, the investigation will proceed similarly, but will be rather more simple.

Let T = the greater time *from* or *to* noon,

τ = the less time.

We shall then have,

$$\sin.\ A = \mp \sin.\ \lambda . \sin.\ \delta + \cos.\ \lambda . \cos.\ \delta . \cos.\ \tau.$$

$$\sin.\ a = \mp \sin.\ \lambda . \sin.\ \delta + \cos.\ \lambda . \cos.\ \delta . \cos.\ T.$$

$$\therefore \sin.\ A - \sin.\ a = \cos.\ \lambda . \cos.\ \delta . \cos.\ \tau - \cos.\ \lambda . \cos.\ \delta . \cos.\ T.$$

$$= \cos.\ \lambda . (\cos.\ \delta . \cos.\ \tau - \cos.\ \delta . \cos.\ T).$$

$\therefore \cos.\ \lambda =$

$$\frac{\sin.\ A - \sin.\ a}{\cos.\ \delta . \cos.\ \tau - \cos.\ \delta . \cos.\ T} = \frac{\sin.\ A - \sin.\ a}{\cos.\ \delta\ (\cos.\ \tau - \cos.\ T)} =$$

$$\frac{2 \cos.\ \frac{1}{2}(A + a) . \sin.\ \frac{1}{2}(A - a)}{2 \sin.\ \frac{1}{2}(T + \tau) . \sin.\ \frac{1}{2}(T - \tau) \cos.\ \delta} = \frac{(\cos.\ \frac{1}{2}(A + a) . \sin.\ \frac{1}{2}(A - a)}{\sin.\ \frac{1}{2}(T + \tau) \sin.\ \frac{1}{2}(T - \tau) \cos.\ \delta} \ldots (2)$$

From one or the other of the expressions (1) or (2), according to the case, the latitude may be *directly* found, without repetition or correction; and the computation is as short as could be desired, and that by the common tables of logarithms. We will now give an example or two, from which the inaccuracy of any indirect method, and the consequent danger of depending on it, will appear very clearly. Let us take one of the examples given in the Nautical Almanac, wrought by the foregoing formula (1)

Alt.

Alt. 5° 36′ A.M. } Interval 3ʰ 0′ } and $\tau = 2^h\ 55'\ 12'' = 43°\ 48'$
Alt. 45 6 A.M. } Decl. ⊙ 12° 0′ N. }

$(A+a) = 50\ 42$
$\frac{1}{2}$ . . . 25 21 . cos. 9·95603

$(A-a) = 39\ 30$
$\frac{1}{2}$ . . . 19 45 . sin. 9·52881

39·48484
29·53498

Lat. 27° 0′ cos. 9·94986

$\tau = 43°\ 48'$
$\frac{1}{2}\ t = 22\ 30$ . . . sin. 9·58284

66 18 . . . sin. 9·96174
cos. 12° 9·99040

29·53498

By the method given in the Nautical Almanac, the latitude, after two corrections, is found to be 27° 59′!

Another example of the second case, wrought by formula (2), will, it is conceived, be not superfluous, and we shall take it from the same.

Alt. 76° 6′ A.M. } Interval 6ʰ 20′ } $T = 5^h\ 39'\ 28''$
Alt. 8 3 P.M. } ☉ Declin. 20° 0′ N. } $\tau = 0\ 40\ 32$

$(A+a) = 84\ 9$
$\frac{1}{2}$ . . . 42 4½ cos. 9·87056

$(A-a) = 68\ 3$
$\frac{1}{2}$ . . . 34 1½ sin. 9·74785

39·61841
29·62375

Lat. = 8° 58′ cos. 9·99466

6 20 0
$\frac{1}{2}(T+\tau) = 3^h\ 10'\ 00'' = 47°\ 30'$ sin. 9·86763
$\frac{1}{2}(T-\tau) = 2^h\ 29'\ 28'' = 37°\ 22'$ sin. 9·78313
cos. $\delta = 20°\ 0'$ . . . . . 9·97299

29·62375

The true latitude then is 8° 58′ N. instead of 10° 1′ found by Dr. Brinkley's indirect method.

I have not noticed any other of the many methods that have been given for the solution of this problem; as their length, indirectness or inaccuracy, or all these together, render them of little use to those who have occasion to reduce them to practice.

Hackney Road, London, Sept. 3, 1825. JAMES BURNS.

XXXI. *Notices respecting New Books.*

Recently Published.

ANTEDILUVIAN Phytology, illustrated by a collection of the Fossil Remains of Plants, peculiar to the Coal Formations of Great Britain. By Edmund Tyrell Artis, F.S.A. F.G.S. author of Roman Antiquities.

This work contains twenty-four engravings of new and interesting fossil plants, with their generic characters, specific differences, descriptions, and localities. Royal 4to.

Flora

Flora Conspicua; A Selection of the most Ornamental Flowering, Hardy, Exotic, and Indigenous Trees, Shrubs, and Herbaceous Plants, for embellishing Flower Gardens and Pleasure Grounds: the generic and specific names, the classes and orders, and distinguishing characters, in strict agreement with Linnæus; the remarks as to cultivation, treatment, and propagation; the particular earth for each plant; its height of growth; month of flowering; and native country. By Richard Morris, F.L.S. &c., author of "Essays on Landscape Gardening," &c.; 8vo. in monthly numbers, each containing four coloured figures.

Elémens de Géométrie théorique et pratique, contenant l'arpentage, la stéréométrie, le jaugeage et la trigonométrie. Par P. A. B. Dupont. 8vo, 148 pages, with 8 plates. Paris, 1825.

Trigonometrische Aufgaben, &c. Trigonometrical Problems, with Analytical and Geometrical Solutions. By Rolla du Rosey and Grabowski. 8vo, with plates. Koenigsberg, 1822.

Annales de Mathématiques pures et appliquées. Par M. Gergonne. tom. 16, No. 1, July 1825.

De la Distribution de l'Electricité dans le cas de trois sphères en contact, dont les deux extrêmes sont égales et les centres sur une même ligne. Intégration des équations relatives à ce cas. Par H. Vernier. 4to, 24 pages. Paris, 1824.

Topographie der sichtbaren Mondoberflaeche. Topography of the Visible Surface of the Moon. By W. G. Lohrmann. No. 1, 4to, 48 pages, with 3 plates. Dresden, 1824.

Découverte de deux mouvemens de la Terre jusqu'ici inconnus, et de méthodes nouvelles pour reconnaître les longitudes sur mer. Par M^e^. P. Guesney. 8vo, 217 pages. Paris.

Handbuch der Meteorologie. Elements of Meteorology. By Kastner. vol. 2, part 1, 640 pages. Erlangen, 1825.

Nouvelle méthode de Perspective, basée sur des combinaisons arithmétiques et géométriques, et application de cette science aux différentes opérations de géodésie, au moyen d'un instrument nommé métroscope. Par le Chevalier de Brunel-Varennes. Part 1. *Perspective linéaire avec l'emploi du métroscope.* 4to, with 10 folio plates. 350 pages. Paris, 1825.

Traité elémentaire de Physique. Par C. Despretz. 8vo. 750 pages, with 14 plates. Paris, 1825.

Ueber das organische Princip in der Erdatmosphære, &c. On the Organic Principle of the Earth's Atmosphere, and on its Meteoric Phænomena. By Dr. C. G. Nees de Esenbeck. 1 vol. 8vo, 120 pages, with a plate. Smalkalde, 1825.

Essai chimique sur les Réactions fulminantes. Par C. J. Brianchon. 8vo. 22 pages. Paris, 1825.

Philosophic

Philosophie chimique, ou Notions générales sur la physique et la chimie. Par L. C. M. Leboullenger, Ingénieur des Mines. 8vo. 260 pages. Paris, 1824.

Preparing for Publication.

The following Proposals have been circulated for Publishing a Work to be called "*Records of Mining.*"

No question has been more frequently asked by those who are interested in the mines of Great Britain, or who wish for information on the art of mining, than "What books are there upon the subject?" To which no other answer can be given than that scarcely any exist, and that, of the very few extant, there are certainly none which describe the mines in their actual state, or which explain the various improvements of later times. That the relations of various matters concerning undertakings so important are devoid of interest is not to be presumed; and, as the eager spirit of speculation which marks the present period will stimulate inquiry into subjects connected with mining, it is probable that a work, having for its object to supply the want in this department of practical science, would meet with encouragement, if conducted in a manner to do justice to the subject. France has long had its *Journal des Mines;* and though that country possesses but few mineral treasures, yet not only has a periodical publication been supported, but the nation has also its establishments for mining education; while England, which produces a greater bulk, at least, of metallic ores than perhaps all other countries combined, possesses neither writers on the subject, nor a school of the art or of the sciences immediately connected with it.

To produce a work such as is now projected is not an easy task, nor would it be prudent to promise much. The persons who know most of the subject are from habit and constant occupation unable to write at length, or to communicate much of the results of their experience; still there are many facts which continually present themselves to observation, all record of which is lost for want of a proper depository for them; and not only is a quantity of valuable matter occurring in the reports and statements of our British mines, but much more will now reach us from those countries in which English capital is employed.

A perfect knowledge of mining ought to correct any tendency to indiscreet speculation: the object of this work will be to advance the one, and thus to control the other. All statements, therefore, will be received with caution, and will be expected to be authenticated by satisfactory evidence: a careful examination

mination will precede the publication of any facts which may be connected with concerns that may seem to invite public attention.

The topics which may be proper for publication are sufficiently various, particularly as it is not intended to restrict the papers within very narrow limits. They may embrace reports upon particular mines, statements of the produce of metals, notices on geological facts, discoveries of ores and minerals, applications of mechanical improvements, and descriptions of existing processes of the treatment of ores and of the operations of smelting, or other modes of reduction. Extracts may be made from foreign or scarce publications which relate to mines: the history of mining would furnish some interesting matter; the natural history or geographical description of countries celebrated for their mines might reasonably be admitted; and a fair and careful investigation of the merits of projected improvements in the machinery or implements destined to the service of the mines would usefully occupy a part of the work. Such a publication may thus be the channel of communication between our miners in the different districts of England and those who are directing operations in other parts of the world, by which the improvements introduced in either region will be made known to the other.

It is proposed to make this publication useful rather than popular; it would therefore be limited in bulk, and regulated as to the times of its appearance entirely by the matter which might offer worthy of being laid before the subscribers. The form would be that of a quarto, to admit of engravings of sufficient size. A part may be occasionally brought out whenever a sufficient portion of matter had accumulated. The price for each part must be regulated by the quantity of letter-press and the number and the quality of engravings. To have the whole well executed, it cannot be what is usually deemed a cheap book; but the intention of the editor is to render it worthy of the subject, and not to make it an object of gain.

The present prospectus is intended to discover whether such a work would be patronized by those for whom only it is intended, namely, persons interested in the subjects which it is designed to illustrate. It is proposed to print only a limited number of copies, and not much to exceed in this respect the demand of regular subscribers. It will be edited under the superintendence of John Taylor, Esq. Treas. G. S.; and in order to determine the extent of the publication, those gentlemen who wish to be furnished with copies are requested to transmit their names to Mr. Arthur Taylor, Printer, No. 40, Basinghall-street.

A new

A new monthly work, in elucidation of that very beautiful and interesting department of science, the Natural History of the Nests and Eggs of British Birds. The descriptions, which are calculated for the naturalist as well as general observer, are intended to comprehend every useful trait of information respecting the nidification, eggs, and incubation of the numerous species of the feathered tribes that inhabit the British Isles; and are throughout accompanied by a series of elegantly-coloured plates, comprehending figures of the eggs of every species, with their most singular varieties, so far as they can be correctly ascertained. The whole exclusively executed from nature, and disposed according to their respective genera, by E. Donovan, F.L.S. W.S. &c. author of the Natural History of British Birds, in ten volumes, and other approved works.

The amount of the plates included in each monthly part, and also the number of the subjects comprised in each plate, will vary from three to four or five, according to the extent of the species appertaining to each genus respectively; every genus being purposely separated from the rest, in order that the whole, when completed, may be distributed into systematic order without difficulty. The descriptions will be also comprised in the same manner into distinct genera; and an index of the whole will be given at the conclusion, for the purpose of a general arrangement.

The subjects will be selected with every attention to that degree of variety which is expected in a periodical production, without being chosen too promiscuously. One plate will be constantly devoted to the representation of subjects which are of more than usual rarity; another to that class of the feathered tribes which are commonly denominated "birds of sport;" and a third to the warblers and other inhabitants of our groves. The indigenous species of our sea coasts, those of our desert heaths and plains, and of our Alpine regions, will appear occasionally, as well as those which are only accidental visitors in the British Isles.

XXXII. *Intelligence and Miscellaneous Articles.*

COMETS.

NO less than *four* or *five* comets have been discovered in the course of the present year, and have engaged the attention of foreign astronomers.

The *first* was discovered by M. Gambart, at Marseilles, on the 19th of May, between the constellations *Cassiopea* and *Andromeda.*

dromeda. It was visible till July 14: and was observed by many astronomers. Its elements have been computed by M. Carlini.

The *second* was discovered by M. Pons, at Marlia, on the 15th of July, in the constellation *Taurus:* and was afterwards observed by him and M. Inghirami at Florence.

The *third* was also discovered by M. Pons, on the 9th of August, in the constellation *Auriga.* This is probably the one now visible near *Aldebaran.*

The *fourth*, which is the most important of the whole, is the celebrated *Comet of Encke*, and appears to have been discovered by M. Valtz at Nismes, on the 13th of July, according to one of the French journals. However this may be, it was certainly seen by M. Plana on the 13th of August; and subsequently by M. Pons, M. Inghirami, and Mr. South: so that no doubt can exist as to the re-appearance of this singular body.

It seems that a *fifth* was discovered by M. Harding on the 23rd of August; but its motion was so rapid to the south, that it is no longer visible in these latitudes.

To the Editor of the Philosophical Magazine and Journal.

Sir,

I hoped to have found in your Magazine for August some observations relating to Pons' or Encke's comet, which has been in a situation favourable for view during the greater part of the last month. It is so curious an object, and so lately ascertained to belong to our planetary system, that every astronomer must be anxious to obtain a sight of it, particularly as this was the first opportunity which has occurred, in our hemisphere, since its orbit was calculated by Encke. This climate, however, is so unfavourable for astronomical pursuits, that, even with the greatest vigilance, many curious phænomena pass unseen; and I fear this may have been generally the case on the present occasion, from the obscure state of the atmosphere in the month of August. I made every exertion to procure a sight of the comet early in the month; but from a long continuance of cloudy weather, or thick haze near the horizon, it was not visible till the 19th. On that night the sky was tolerably clear, though the stars did not shine with a steady light, so that the comet did not appear under very advantageous circumstances. At three o'clock in the morning (15th) it formed nearly an equilateral triangle with *Pollux* and σ *Geminorum*, the comet being to the eastward. The appearance of it was like a bright nebule, and resembled that, well known,

near

near the head of *Aquarius*. I used a five-feet achromatic, with a power of 80, and saw the object very distinctly. It was about 16° above the horizon. On August the 23d I again saw it, at the same hour as before. It had moved nearly 8° from its former situation on the 19th. On the 25th, the twilight and a considerable haze near the horizon obscured the comet; and no other opportunity of seeing it offered during the remainder of the month. From the remarks I made on the 19th, it seemed to be considerably further advanced than the position given in Bode's Table. In the winter of 1828 we may expect a return, and a more commodious view of this extraordinary body.

Your obedient servant,

Winterdyne, Sept. 13, 1825. M.

To the Editor of the Philosophical Magazine and Journal.

Sir,

On Thursday morning of the 8th instant, about 2 o'clock, I discovered a comet in the face of *Taurus*, nearly opposite to *Aldebaran* and the *Hyades*, with a Dollond's night-glass. It was not then visible to the naked eye, nor had it the least appearance of a nucleus. It appeared like an oval burr of vapourish light, similar to some of the nebulæ. With achromatic or reflecting telescopes, of small or large power, nothing could be made of it; but the less the magnifying power the better. The right ascension was 59·10 and north declination 16·30. The tail in opposition to the sun.

Since that time the weather has been so exceedingly unfavourable, that only few and short opportunities have presented themselves for further observation. This morning (of the 18th) I occasionally had views of it, as it is now become very conspicuous to the naked eye; so that from two till four I was enabled to notice the progress it had made, and its apparent path. Its direction is nearly from north to south; and it is evidently approaching the sun in its perihelion. To the naked eye the tail appears more than a degree in length, but still no nucleus is visible either with glasses or without: the head is well defined, spherical, and of an uniform phosphoric appearance. The tail is irregular, and the fixed stars are easily seen through it.

Since I first discovered it, the advance from north to south has been from 10 to 12 degrees. It now forms nearly an equilateral triangle with the *Pleiades* and *Aldebaran*, or a very small triangle with the star marked λ and a smaller one. From the short times of seeing it I think the motion irregular, being more rapid at one period than another. When I first ob-

served this comet it was very near the ecliptic. As the weather will soon be more favourable, plenty of good opportunities may be had of tracing its apparent path. The dark nights will render it still more distinct, independent of its nearer approach towards the earth.

About three years ago, I discovered a small comet in the *Great Bear*, with the same telescope, which I never observed noticed in any of the journals, though visible for some weeks.

Whatever further remarks I may be enabled to make, under the privation of health, I will send you.

I am your obedient servant,

Ipswich, 18th September, 1825. J. Acton.

P. S. Right ascension 54·30, North declination 10·10, when last seen.

To the Editor of the Philosophical Magazine and Journal.

Sir,

I hasten to communicate to you, that on the 15th inst. being at Hartfield in Sussex, I perceived, about midnight, during a very clear interval, a luminous phænomenon with which I was unacquainted, in the right shoulder of the *Bull*. On looking at it with a four-feet refracting telescope, it appeared to be a comet, with a tail of considerable length, and a nucleus about the apparent size of a star of the third or fourth magnitude. Being just on the point of leaving home, I did not make any further observations on it. Any collateral remarks of your correspondents will be interesting.

I have the honour to remain, yours, &c.

Sept. 23, 1825. T. Forster.

PLANETARY ANALOGIES.

The ratio of the *periodic times* of any two planets, divided by the ratio of their *mean distances*, is equal to the inverse ratio of their *projectile forces*, or equal to the 4th root of the inverse ratio of their *gravitating forces*, and which is constantly equal to the square root of the ratio of their *mean proportional distances*.—Required, a Demonstration?

Sept. 1825. Alfred.

NATIVE SELENIURET OF LEAD AND NATIVE SULPHURET OF SELENIUM.

"As Professor Stromeyer has favoured me with a copy of his and Professor Hausmann's paper on a native seleniuret of lead, which was lately read before the Royal Society of Göttingen, I beg leave to send you a notice concerning it. The mineral was sent by M. Bauersachs, of Zellerfeld in the Hartz,

Hartz, to Professor Hausmann, with the observation that it contained selenium. It was found some years since in the St. Lawrence mine near Clausthal; and M. Bauersachs, who at that time regarded it as a distinct species, termed it cobaltic galena; under which name Professor Hausmann introduced it into his Mineralogy. The mineralogical description of it, as given by Professor Hausmann, is as follows: Externally it bears considerable resemblance to fine granular galena, though its colour is clearly different, having a cast of blue like molybdena. A crystalline texture is quite distinct, but from the minuteness of its crystals it has hitherto been impossible to ascertain their precise form. Cubic and triangular surfaces were observed; but whether they correspond or not to those of galena could not be determined. A similar remark applies to its cleavages, of which there appear to be several. It is less hard than galena; and its density is 7·697. It becomes negatively electric from friction, like galena. It is readily decomposed, before the blowpipe, on charcoal. Besides the usual phænomena arising from the presence of lead, the odour of decayed horseradish may be perceived; and a reddish-brown matter is deposited round the assay on the cool parts of the charcoal. The ore shines with a clear blue light while the blowpipe flame is playing upon it. It communicates a pale-blue colour to borax, indicative of a little cobalt. When heated by means of a spirit-lamp, in a clear glass tube closed at one end, the selenium almost instantly sublimes, forming a red ring within the tube, at the open extremity of which its peculiar odour is very perceptible. On heating the tube to redness, the ore fuses, the red ring partially disappears, and is succeeded by a white crystalline deposit. This deposit reddens litmus paper, is deliquescent, and has all the properties of selenic acid. These characters, which I have myself witnessed on a specimen sent me by Professor Stromeyer, are very distinct. Nitric acid acts readily upon the ore even in the cold. The lead is first attacked, the selenium separating in substance in red flocculi: by the aid of the heat these also are oxidized, selenic acid being generated. The solution, when complete, has a pale rose-colour, owing to the presence of cobalt; but the nicest test could detect neither sulphuric acid nor any other substance. The analysis was performed by the following method:—After dissolving the ore completely in nitric acid, the oxide of lead was precipitated by sulphuric acid, the operation being conducted at a boiling temperature, to prevent the precipitation of any seleniate of lead. The filtered solution was then concentrated by evaporation, and selenium thrown down by sulphate of ammonia

and

and sulphurous acid. The cobalt was next separated by the hydrosulphuret of ammonia. The proportion, as drawn from the mean of three nearly corresponding analyses, is,

Lead	70·98
Cobalt	0·83
Selenium . . .	28·11
	99·92

" With respect to the atomic constitution of this ore, Prof. Stromeyer remarks, that 'its constituents are combined precisely in the proportion of their equivalents, and the quantity of the selenium corresponds not only to the lead, but also to the cobalt; and that, therefore, both metals are to be regarded as in combination with selenium. The seleniate of lead, too, agrees with the sulphuret of that metal in this respect,—that, when both its constituents are oxidized, the selenic acid and oxide of lead are in the precise proportion to form a neutral seleniate of lead, just as the oxidation of galena gives rise to a neutral sulphate of lead. The discovery of such a compound is therefore to be anticipated whenever a native seleniuret of lead exists.'—I take this opportunity to mention that the native sulphuret of selenium which Professor Stromeyer detected among the volcanic products of the Lipari Isles (of which I communicated a short notice some months since), was found among a mixed sublimed mass of muriate of ammonia and sulphur. It was disposed in layers, and, from its brownish-yellow colour, gives rise to the supposition that the muriate of ammonia at such parts contained iron. A superficial examination proved, however, that no iron was present; and this observation led to the detection of the selenium. In a letter which I have just received from Professor Stromeyer, he informs me that another ore has been found in the Hartz, containing the seleniurets of lead, copper, silver, and mercury, with the examination of which he is at present occupied."—*Letter from Dr. Turner in the Edin. Phil. Journ.*

ANALYSES OF SEVERAL MINERALS: BY PROF. GMELIN.

" Since my last to you I have been much occupied with analyses, and shall give you some of the results obtained.

" I have discovered a beautiful mica in large laminæ to be a crystallized Lepidolite—that is to say, to contain lithion. It is composed of

Silica	52·254
Alumina	28·345
Ox. of manganese .	3·602
Potash	6·903
Lithion	4·792
Fluoric acid	3·609
	99·505

" This

"This mica has a beautiful rose-red colour, and occurs near Penig, in Saxony, together with amblygonite, topaz, albite, schorl, &c.

"Most of the minerals that occur in this place contain lithion; as for instance, a fine variety of quartz, lithomarge, andalusite, &c. I shall publish the analyses of all these minerals in the Edinburgh Philosophical Journal, and it will give me much pleasure to send you specimens of each.

"While on this subject, allow me to say that I have discovered a very useful test for lithion, before the blowpipe; viz. the flame assumes a very fine purple colour,—but the flame of an oil lamp should be used, and not that of a tallow candle. By means of the latter the colour of the flame is not so decided.

"By an analysis of Helvin, a very scarce mineral, I have discovered glucine to be a constituent of it. It consists of

Silica	33·258
Glucine	12·089
Oxydule of manganese	31·817
Protoxide of iron	5·564
Sulphuret of manganese	14·000
	96·728
Loss by ignition	1·555

"The Latrobite of Mr. Brooke (diploite of Bresthaupt) is composed, according to my analyses, of

Silica	44·653
Alumina	36·814
Lime	8·291
Ox. of manganese	3·160
Manganese with ox. of manganese	0·628
Potash	6·575
	100·000
Gain	·121 "

Letter from Prof. Gmelin to Dr. Van Rensselaer in Silliman's Amer. Journ.

PREHNITE.—OLIVINE.

A very elaborate examination of several varieties of prehnite has been made under the direction of L. P. Walmstedt, professor of chemistry in the university of Upsal, by MM. P. F. Wahlberg, J. A. Hœger, and S. A. Varenius, candidates for the philosophical degree. The very discordant results obtained by the different chemists who had analysed this mineral, induced these gentlemen to make it the subject of their experiments. The analyses of two varieties of prehnite made by Gehlen,

Gehlen, and published in 1813, afforded results similar to each other—very different, however, from the results obtained by other analysts. The analyses of several varieties, which were subjected to the rigid examination here noticed, all afforded results which coincided very nearly with the analyses of Gehlen. It appears therefore that the accuracy of Gehlen's analyses is pretty fully established, together with the fact that the several varieties of prehnite differ in their chemical composition much less than has been generally supposed.

Another candidate for the same degree, Mr. P. N. Sevén, has examined a specimen of olivine from Mount Somme, near Naples. The composition of this substance, as determined by Klaproth, would seem hardly to admit of its being associated with chrysolite.

The following are the results of two analyses made by Mr. Sevén.

Silica	40·08	Oxyg. 20·16	40·16	Oxyg.	20·20
Magnesia . . .	44·24	——— 17·12	44·87	———	17·37
Oxid. ferrosum	15·26	——— 3·47	15·38	———	3·50
——— manganos.	0·48		0·10		
Alumina	0·18		0·10		
	100·24		100·61		

These results, so widely different from those of Klaproth's analysis, clearly justify the association of olivine in the same species with chrysolite. C. H.—*American Journ. of Science.*

NEW VARIETY OF ORTHITE, AND A NEW MINERAL RESEMBLING FELDSPAR, &c.

Extract of a letter to Dr. Silliman from Prof. Berzelius of Stockholm: dated July 3, 1824.

"Nothing particularly remarkable has occurred here in mineralogy, except that in the midst of the city of Stockholm two minerals have been discovered, one of which appears to me new, and the other is the orthite, before found only at Finbo, near Fahlun. For the purpose of building a church upon one of the six islands which form our city, they cut down a part of a mountain in which these minerals were found. We afterwards discovered that they are found every where in the granite about Stockholm: as yet they are not very numerous, but probably will, in the progress of time, be found abundantly. In a box of minerals which I am sending from Count Warzlmeister to Dr. Torrey, I have sent for you two specimens: the orthite of two varieties, one of which perfectly resembles the gadolinite, for which we at first took it; and the other, with a granular fracture, has a false appearance of yttrotantalite. The new mineral has the most perfect resemblance

blance to feldspar, from which it nevertheless differs, in consequence of its great fusibility, and by the property which it possesses of being very much augmented in volume by the action of the blowpipe when it is heated to redness. There are two varieties, one of which is white, and the other greenish. The result of a hasty analysis of it, which I have performed, gives the same composition as triphane and spodumene,—with this difference, that it contains sodium instead of lithium. The greenish variety contains also lime and a little magnesia. I presume that this may be the same mineral as the killinite, *Cleaveland*, vol. i. p. 309.

Mr. Walmstedt, professor of chemistry at Upsal, has performed a series of researches upon prehnite, of which, as it is in Latin, I take the liberty to send you a copy.—*Amer. Journ. of Science.*

HAZEL NUTS FOUND IN A SINGULAR STATE AT A GREAT DEPTH.

We have been kindly presented, by Sir John Hay, Bart., of Smithsfield and Hayston, with a packet of hazel nuts, found upon one of his farms at Bonnington, about one mile south from Peebles. The nuts were found in a bog, about eight feet below the surface. The top soil was three feet of meadow clay, beneath which was a layer of grayish-coloured gravel about four feet and a half thick. The bottom of the bog consisted of a mixture of gray sand and brown moss, with some branches of stumps of trees, quite rotten. The nuts were found nearest the bottom of this substance. The bog is part of a meadow about 1500 yards long, by about from 300 to 600 feet broad, having a declivity of about 1 foot in 400.

Upon opening these nuts, we were surprised to find that *the kernel in all of them had entirely disappeared, though the membrane which inclosed it, and the nut itself, were as entire as if the nut had been fresh and ripe.* By opening the nut carefully, the membrane could be taken out in the form of a perfect bag, without the least opening. The substance of the kernel must therefore have escaped through the membrane and the shell in a gaseous form, or must have passed through them when decomposed or dissolved by water. In some of the nuts that had not arrived at maturity the bag was very small, and was surrounded, as in the fresh nut, with the soft fungous substance, which had resisted decay.

SOME ACCOUNT OF THE MODE IN WHICH THE BOA CONSTRICTOR TAKES ITS PREY, AND OF THE ADAPTATION OF ITS ORGANIZATION TO ITS HABITS. BY W. J. BRODERIP, ESQ. F.L.S.&C.

In March last Mr. Cop of the Lion Office, in the Tower, sent to inform me that one of these reptiles had just cast his skin, at which period, they, in common with other serpents, are most active and eager for prey. Accordingly I repaired with some friends to the Tower, where we found a spacious cage, the floor of which consisted of a tin case covered with red baize and filled with warm water so as to produce a proper temperature.—There was the snake *positis novus exuviis*, gracefully examining the height and extent of his prison, as he raised, without any apparent effort, his towering head to the roof and upper parts of it, full of life, and brandishing his tongue.

A large buck rabbit was introduced into the cage. The snake was down and motionless in a moment. There he lay like a log without one symptom of life, save that which glared in the small bright eye twinkling in his depressed head. The rabbit appeared to take no notice of him, but presently began to walk about the cage. The snake suddenly, but almost imperceptibly, turned his head according to the rabbit's movements, as if to keep the object within the range of his eye. At length the rabbit, totally unconscious of his situation, approached the ambushed head. The snake dashed at him like lightning. There was a blow—a scream—and, instantly, the victim was locked in the coils of the serpent. This was done almost too rapidly for the eye to follow: at one instant the snake was motionless;—in the next, he was one congeries of coils round his prey. He had seized the rabbit by the neck just under the ear, and was evidently exerting the strongest pressure round the thorax of the quadruped; thereby preventing the expansion of the chest, and, at the same time, depriving the anterior extremities of motion. The rabbit never cried after the first seizure:—he lay with his hind legs stretched out, still breathing with difficulty, as could be seen by the motion of his flanks. Presently he made one desperate struggle with his hind legs; but the snake cautiously applied another coil with such dexterity as completely to manacle the lower extremities, and in about eight minutes the rabbit was quite dead. The snake then gradually and carefully uncoiled himself; and finding that his victim moved not, opened his mouth, let go his hold and placed his head opposite to the fore part of the rabbit. The Boa generally, I have observed, begins with the head; but, in this instance, the serpent, having begun with the fore legs, was longer in gorging his prey than usual; and,

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in consequence of the difficulty presented by the awkward position of the rabbit, the dilatation and secretion of lubricating mucus was excessive. The serpent first got the fore legs into his mouth; he then coiled himself round the rabbit, and appeared to draw out the dead body through his folds; he then began to dilate his jaws, and, holding the rabbit firmly in a coil as a point of resistance, appeared to exercise, at intervals, the whole of his anterior muscles in protruding his stretched jaws and lubricated mouth and throat at first against, and soon after gradually upon and over his prey. The curious mechanism in the jaws of serpents which enables them to swallow bodies so disproportioned to their apparent bulk is too well known to need description; but it may be as well to state that the symphysis of the under jaw was separated in this case, and in others which I have had an opportunity of observing. When the prey was completely ingulphed, the serpent lay for a few moments with his dislocated jaws still dropping with the mucus which had lubricated the parts;—and, at this time, he looked quite sufficiently disgusting. He then stretched out his neck, and at the same moment the muscles seemed to push the prey further downwards. After a few efforts to replace the parts, the jaws appeared much the same as they did previous to the monstrous repast.—*Zoological Journal.*

MEXICAN MINES.

The following is an extract of a private letter from Mexico; its contents are of some interest, particularly in reference to the progress which is making in working the several mines belonging to the different Companies formed in this country:—

Mexico, July 9, 1825.—You will express surprise, perhaps, at my not sending you more political news, but it is only because there is none to send. The congress, it is expected, will meet on the first of next month, and then, perhaps, we may find some gossip to send you. I find this city more agreeable as a residence than any part of South America I have been in.

I have been naturally led, since I have been here, to make some comparison between this country and that other section of South (or, I should say, Spanish) America, of which, as you are aware, I know the most—I mean Colombia; and I should say, that although, as regards their separation from Spain, it is as complete in the one country as in the other; yet, in point of national energy and national feeling all together, Colombia is more advanced than Mexico. Fourteen years of warfare, difficulty, and suffering, have developed and matured a national character in Colombia in a greater degree than has occurred here, precisely because the same causes have not pre-

vailed to the same extent. However, causes of a different kind are at work in this country now, which, it is to be hoped, will tend every day towards its advancement in civilization. The principal of these, and the one which now engages most of the public attention, is the working of the mines. You have little idea in England of the benefit which the mining districts of Mexico are already feeling from the putting their industry in motion. All the people are employed. There is a demand every where for horses, provisions, iron, paper, and every thing that the miners want for their operations. The three principal English Companies have apparently taken different courses of proceeding, each and all of which are the subject of conversation and criticism here. The Real del Monte Company, who have got the immense mines of Count Regia, go upon the plan of employing steam-engines. Their engines are arrived on the coast, I hear, and it is said they expect to get them conveyed to the mines, and to have them erected and be ready for work by next spring. The question that is propounded among the learned in these matters is, whether, after the engines have got to work and done their business by draining the mines, the same result could not have been obtained by the means of the country, and at less expense and with less delay than by steam-engines? The expense of conveying the steam-engines, and repairing the roads for that purpose, is very great.

The Anglo-Mexican Company, which has its mines principally in Guanaxuato, has followed a different plan. They have adopted the Mexican system of *malacates*, making some improvements in them; and by means of these they are draining the great mine of Valenciana at the rate of 10,000 tons of water per week. I was perfectly astonished to find the power of these things. Eight of them, I understand, are now established on the great shaft of Valenciana, and are at work night and day. Each *malacate* takes twelve horses, which are relieved every six hours. The expense must be great, but the mine is yielding ore every week. Both this Company and the Real del Monte have had out a good many Cornish miners.

The next great Company is the United Mexican. It seems to be the policy of this Company, as far as I can learn, to work entirely with the means of the country, capital and management being the great articles they supply. They have got some very valuable mines in Guanaxuato, Zacatecas, and other districts.—The whole of these Companies are under excellent management. The one last mentioned, the United, is under the direction of Don Lucas Alaman, minister for foreign affairs,

affairs, a man of great value to this country—able, upright, scientific, and with a great turn for mining. He is assisted by one or two London merchants, who, with him, form a Board of Management here. The Anglo-Mexican is under the direction of Mr. Williamson, who resides at Guanaxuato, a gentleman who, some time since, undertook the working of the King of Persia's mines, but was obliged to leave that country in consequence of the treatment he received there. He is a man of great talent, judgement, and exertion. The Real del Monte is under the direction of Captain Veitch, an officer of engineers, and son [brother] of a physician in London of that name. He appears to be a very able man, equal to the trust reposed in him, and is very much esteemed. Besides these there are three or four other smaller Companies. The effect of their several labours must be soon felt in this country, and I guess it will not be many years before it is felt in Europe.—*Courier*, Sept. 26.

Note.—The information contained in the above letter agrees in the main, we understand, with other accounts of these important undertakings which have from time to time been received. It may, however, be calculated to convey an erroneous impression in regard to a circumstance which forms one of its principal features, viz. "the different courses of proceeding" taken by the three principal Companies, and which are described as being "the subject of conversation and criticism." We believe that these different courses have been adopted, not on account of any difference of opinion respecting the great advantages to be derived from the use of steam-engines, with which all Cornish miners must be well acquainted,—but the real state of the case is, that these mines, which it is said "to be the policy to work entirely with the means of the country," are destitute of fuel, whereas the Real del Monte has now a forty years' supply of wood, with a territory and climate where it is rapidly reproduced. On this account the engines which had been ordered for other Companies, whose agents found they could not make use of them, have been bought by the Real del Monte Company.

With regard to "obtaining the same result by the means of the country," we believe many of the great mines in Mexico are in the same case as those of Cornwall were some time back, where they had nearly come to a stand upon the old means. "A failure in produce, similar to that experienced in the mines of Cornwall 60 or 80 years ago, before the application of the steam-engine, had already [before the Revolution] given a considerable check to the prosperity of the mines; and in the year 1810, at the commencement of the civil commotions

motions in Mexico, they experienced a fatal blow by the interruption to industry produced by internal war*." To what a vast extent the application of the steam-engine has raised the produce of mines in Cornwall, which had been nearly or wholly abandoned for the want of means to keep out the water, no one in this country can be at a loss to learn. Of the Mexican system of *malacates* alluded to by the writer, we have the following account by Humboldt:

"We have already spoken of the truly barbarous custom of drawing off the water from the deepest mines, not by means of pump apparatus, but by means of bags attached to ropes which roll on the cage of a whim. The same bags are used in drawing up the water and the ores: they rub against the walls of the shafts, and it is very expensive to keep them in repair. At the Real del Monte, for example, these bags only last seven or eight days; and they commonly cost five, and sometimes seven and eight shillings a piece. A bag full of water, suspended to the cage of a whim with eight horses (*malacate doble*), weighs 1250 pounds: it is made of two hides sewed together. The bags used for the whims called *simple*, those with four horses (*malacates sencillos*), are only half the size, and are made of one hide. In general the construction of the whims is extremely imperfect; the bad custom also prevails of forcing the horses, by which they are made to go at far too great a speed. I found this speed at the shafts of San Ramon at Real del Monte no less than ten feet and a half per second; at Guanaxuato, in the mine of Valenciana from thirteen to fourteen feet; and every where else I found it more than eight feet. Don Salvado Sein, Professor of Natural Philosophy at Mexico, has proved in a very excellent paper on the rotatory motion of machines, that, notwithstanding the extreme lightness of the Mexican horses, they produce only the *maximum* of effect on the whims, when exerting a force of 175 pounds they walk at a pace of from five to six feet in the second.

"It is to be hoped that pumps, moved either by horse-engines of a better construction, or by water wheels, or by pressure engines, will at last be introduced in the mines of New Spain. If wood and coal, which has only yet been discovered in New Mexico, should be found sufficiently abundant for employing the steam-engine, the use of it would be of great advantage in the inundated mines of Bolaños, as well as in those of Rayas and Mellado.

"It is in the draining the mines of water that we particularly feel the indispensable necessity of having plans drawn up by

* Selections from Humboldt relating to Mexico, by John Taylor, Esq. Introd. p. vii.

subter-

subterraneous surveyors (*geometres*). Instead of stopping the course of the water, and bringing it by the shortest road to the shaft where the machines are placed, they frequently direct it to the bottom of the mine, to be afterwards drawn off at a great expense. In the district of mines of Guanaxuato nearly two hundred and fifty workmen perished in the space of a few minutes on the 14th of June 1780; because, not having measured the distance between the works of San Ramon and the old works of *Santo Christo de Burgos*, they had imprudently approached this last mine while carrying on a drift in that direction. The water of which the works of Santo Christo were full, flowed with impetuosity through this new gallery of San Ramon into the mine of Valenciana. Many of the workmen perished from the sudden compression of the air, which, in taking vent, threw to great distances pieces of timber and large masses of rock. This accident would not have happened, if, in regulating the operations, they could have consulted a plan of the mines.

"After the picture which we have just drawn of the actual state of the mining operations, and of the bad management which prevails in the mines of New Spain, we cannot be astonished at seeing works which for a long time have been most productive, abandoned whenever they reach a considerable depth, or whenever the veins appear less abundant in metals. We have already observed, that in the famous mine of Valenciana the annual expenses rose in the space of fifteen years from 90,000*l.* to 180,000*l.* sterling. Indeed, if there be much water in this mine, and if it require a number of whims to draw it off, the profit must, to the proprietors, be little or nothing. The greater part of the defects in the management which I have pointed out, have been long known to a respectable and enlightened body, the *Tribunal de Mineria* of Mexico, to the professors of the School of Mines, and even to several of the native miners, who, without having quitted their country, know the imperfection of the old methods: but we must repeat here, that changes can only take place very slowly among a people who are not fond of innovations. It is a prejudice to imagine that the wealth of the mines of New Spain renders unnecessary the intelligence and the œconomy which are requisite in other mines*."

The only mine in Mexico in which pumps of any kind had been tried is that of Moran, in the district of Real del Monte; of which M. Humboldt gives the following account:

"The mountains of the district of mines of Real del Monte

* Selections, &c. p. 194—197.

contain

contain beds of porphyry, which, with respect to their relative antiquity, differ a good deal from one another. The rock which forms the roof and the wall of the argentiferous veins is a decomposed porphyry, of which the base sometimes appears clayey, and sometimes analogous to splintery hornstone. The presence of hornblende is frequently announced, merely by greenish stains intermingled with common and vitreous feldspar. At very great elevations, for example in the beautiful forest of oak and pine of Oyamel, we find porphyries with a base of pearl-stone, containing obsidian in layers and nodules.

"What relation exists between these last beds, which several distinguished mineralogists consider as volcanic productions, and the porphyries of Pachuca, Real del Monte, and Moran, in which nature has deposited enormous masses of sulphuret of silver and argentiferous pyrites? This problem, which is one of the most difficult in geology, will only be resolved when a great number of zealous and intelligent travellers shall have gone over the Mexican Cordilleras, and carefully studied the immense variety of porphyries which are destitute of quartz, and which abound both in hornblende and vitreous feldspar.

"The district of mines of Real del Monte does not display—as at Freiberg in Saxony, Derbyshire in England, or as in the mountains of Zimapan and Tasco in New Spain—a great number of rich veins of small size, on a small tract of ground. It rather resembles the mountains of the Hartz, and Schemnitz, in Europe, or those of Guanaxuato and Potosi in America, of which the riches are contained in a few mineral depositions of very considerable dimensions. The four veins of Biscaina, Rosario, Cabrera, and Encino, run through the districts of Real del Monte, from Moran and Pachuca, at extraordinary distances, without changing their direction, and almost without coming in contact with other veins which traverse or derange them.

"The *veta de la Biscaina*, of less considerable dimensions, but perhaps still richer than the vein of Guanaxuato, was successfully wrought from the sixteenth to the beginning of the eighteenth century. In 1726 and 1727, the two mines of Biscaina and Xacal still produced together 333,969 ounces of silver. The great quantity of water which filtrated through the crevices of the porphyritic rock, joined to the imperfection of the means of drawing it off, compelled the miners to abandon the works when they were yet only 65 fathoms in depth. A very enterprising individual, Don Joseph Alexandro Bustamente, was courageous enough to undertake a level near Moran; but he died before completing this great work, which

is

is 7715 feet in length from its mouth to the point where it crosses the vein *de la Biscaina.* The direction of this vein is hor. 6; and its inclination is 85° to the south: its extent is from 13 to 19 feet. The direction of the porphyry of this district is generally hor. 7-8, with an inclination of 60° to the north-east, particularly in the road from Pachuca to Real del Monte. The level is at first cut through the solid rock (*querschlagsweise*) in a direction of hor. 7, towards the west; but further on it takes its way over three different veins, hor. 11-12, of which one alone, the *veta de le Soledad**, has furnished a sufficiency of silver ores to pay all the expenses of the undertaking. The level was only finished in 1762, by Don Pedro Tereros, the partner of Bustamente. The former, known by the title of Count de Regla as one of the richest men of his age, had already drawn, in 1774, a net profit of more than 1,041,750*l.* from the mine of Biscaina. Besides the two ships of war which he presented to King Charles the Third, one of them of 120 guns, he lent 208,350*l.* sterling to the Court of Madrid, which have never yet been repaid him. He erected the great works of Regla at an expense of 416,700*l.* sterling; and he purchased estates of an immense extent, and left a fortune to his children, which has only been equalled in Mexico by that of the Count de la Valenciana.

"The level of Moran traverses the vein of *La Biscaina*, in the San Ramon shaft, at a depth of 115 fathoms below the level of the surface on which the whims are placed. The profit of the proprietor has been annually diminishing since 1774. In place of driving levels for trial, to discover the vein on a great extent, they continued their sinking to a depth of nearly 53 fathoms below the level. At that depth the vein preserved its great wealth in sulphuret of silver mixed with native silver; but the abundance of water increased to such a degree that 28 whims, each of which required more than 40 horses, were not sufficient to draw it off. In 1783, the weekly expense amounted to 1875*l.* After the death of the old Count de Regla, the works were suspended till 1791, when they ventured to re-establish all the whims. The expense of these machines, which drew up the water, not by means of pumps, but by bags suspended by ropes, then amounted to more than 31,252*l.* per annum. At length they reached the deepest point of the mine, which according to my measurements is

* "It is believed that this vein is the same with that which M. D'Eluyhar began to work in the pit of Cambrera, at Moran. It appeared to me, however, that the *veta de Cambrera* is rather the same with that of *Santa Brigida*, and that its principal wealth is to be found in following it towards the mine of Jesus."

only 1064 feet above the level of the lake of Zumpango; but the ores which they extracted did not compensate the expense of the process, and the mine was again abandoned in 1801.

"It is surprising that they never thought of substituting for this wretched plan of drawing off the water by bags proper pump apparatus, put in motion by horse whims, by hydraulic wheels, or by machines moved by a column of water (*colonne d'eau*). A level begun at Pachuca, or lower down towards Gazave in the valley of Mexico, would have exhausted the mine of Biscaina at the pit of San Ramon, for a depth of 202 fathoms. The same object could be attained at less expense by following the project of M. D'Elhuyar, in placing the mouth of a new level near Omitlan, in the road which leads from Moran to the place of amalgamation at Regla. This last level, before reaching 12,466 feet in length, would cut the vein of Biscaina.

"The very wise plan which the Count de Regla at present follows is, to leave off the clearing of the old works, and to investigate the mineral repository in points where it has never yet been worked (*in unverfahrenem felde*). In studying at Real del Monte the surface and undulations of the ground, we observe that the vein of Biscaina has furnished for three centuries its greatest riches from a single spot; that is to say, from a natural hollow (*enfoncement*) contained between the shafts of Dolores, Joya, San Cayetano, Santa Teresa, and Guadalupe. The shaft from which the greatest quantity of silver ores has been extracted is that of Santa Teresa. To the east and west of this central point the vein is contracted for a distance of more than 1300 feet. It preserves its primitive direction, but becomes destitute of metals, and reduced to an almost imperceptible vein. For a long time it was believed that the vein of Biscaina was insensibly lost in the rock; but they discovered in 1798 very rich metals, at a distance of more than 1640 feet, to the east and west of the centre of the old works. They then sunk the shafts San Ramon and San Pedro, and discovered that the vein resumed its old power, and that an immense field was opened to new undertakings. When I visited the mines in the month of May 1803, the San Ramon shaft was then only 16 fathoms in depth; and it will be nearly 131 fathoms to the bottom of the level of Moran, which is itself still distant 147 feet from the point which corresponds to the intersection of the new shaft, and the roof of the level. In its present state, the mine of the Count de Regla annually yields more than from 30,000 to 40,000lbs. troy of silver.

"The vein of Biscaina contains in the points of the principal mines milk-quartz, which frequently passes into splintery horn-

hornstone, amethyst, carbonate of lime, a little sulphate of barytes, sulphuret of silver mixed with native silver, and sometimes prismatic black silver (*sprödglaserz*), deep-red silver, galena, and iron and copper pyrites. The same silver ores are found near the surface of the ground in a state of decomposition, and mixed with oxide of iron, like the *pacos* of Peru. Near the San Pedro shaft, the pyrites are sometimes richer in silver than the sulphuret of silver.

"The mines of Moran, formerly of great celebrity, have been abandoned for 40 years on account of the abundance of water, which could not be drawn off. In this district of mines, which is in the vicinity of that of Real del Monte, near the mouth of the great level of Biscaina, there was placed in 1801 a machine *à colonne d'eau*, of which the cylinder is 10·23 inches in height, and 6·29 in diameter. This machine, the first of the kind ever constructed in America, is much superior to those of the mines of Hungary. It was executed agreeably to the calculations and plans of M. del Rio, professor of mineralogy in Mexico, who has visited the most celebrated mines of Europe, and who possesses at once the most solid and the most various acquirements. The merit of the execution is due to M. Lachausée, a Brabant artist of great talents, who has also fitted up for the School of Mines of Mexico a very remarkable collection of models, for the use of students of mechanics and hydrodynamics. It is to be regretted that this fine machine, in which the regulator of the suckers is put in motion by a particular mechanism, was placed in a situation where there is great difficulty in procuring a sufficiency of water to keep it going. When I was at Moran, the pumps could only work three hours a day. The construction of the machine and the aqueducts cost 10,937*l.* sterling: they did not at first calculate on more than half of the expense, and they imagined the mass of water to be very considerable; but the year in which the water was measured being exceedingly rainy, it was believed to be much more abundant than it actually was. It is to be hoped that the new canal which was going on in 1803, and which will be 16,404 feet in length, will remedy this want of water, and that the vein of Moran (hor. $9\frac{4}{8}$ inclined 84° to the north-east) will be found as rich at great depths as the shareholders of the mine suppose. M. del Rio, on my arrival in New Spain, had no other view but that of proving to the Mexican miners the effect of machines of this nature, and the possibility of constructing them in the country. This object has been in part attained; and it will be much more evidently attained when such a machine shall be placed in the mine of Rayas at Guanaxuato, in that of the Count de Regla at Real

del Monte, or in those of Bolaños, where M. Sonneschmidt counted nearly 4000 horses and mules employed in moving the whims."

The benefits described as being conferred on the mining districts by putting their industry in motion were thus justly anticipated in the Report to Congress of Don Lucas Alaman, minister for foreign affairs, dated Nov. 1st, 1823.

"It is a principle admitted by all writers on political œconomy, that the most direct encouragement that can be given to agriculture and to industry, is to facilitate the consumption of the produce of the one, and the sale of the manufactures of the other. If the mines be considered amongst us under this point of view, it will be found that nothing contributes so much as they do to the prosperity of those essential branches of the public riches. The great number of people that are occupied in them, the animals that are employed in the working of the machinery and in transporting the ores, the consumption that arises therefrom of grain, as well as of soap, paper, iron, &c., give a powerful impulse to agriculture, the arts, and to commerce. If practical illustration be necessary to prove those facts, which are doubted only by men whose minds are preoccupied by the paradoxical assertions of systematic œconomists, they may be found on a comparison of the state of our mining provinces, such as Guanaxuato and Zacatecas, previous to the year 1810 and at the present period. Abundance and prosperity then reigned throughout both of them. The agriculturist found in those famous *reales* (districts) a ready and certain market for his produce; the smith, the carpenter, the mason, a constant employment for his industry; the merchant, an extensive consumption for the goods which he introduced; and the treasures drawn from the bowels of the earth were distributed throughout and revivified the most distant provinces in payment for the soap, wood, salt, magistral, horses and mules, that were brought from all parts. The nature of our ores is also a powerful cause of these happy results: they are generally poor in metal, aud most abundant in quantity, and require for their manufacture a great quantity of machinery and ingredients; and it may therefore be said, that the miner merely draws forth funds to distribute them freely among the labourers, merchants, and artisans; and we must naturally conclude that the prosperity of these classes depends principally upon the impulse given to them by the mines, which in our notion are thus the acting principle of all the other branches of industry."

* Selections, &c. p. 274—280. † Ibid. Introd. p. xxx. xxxi.

AFRICA.

Portsmouth, Sept. 24.—On Monday the Nassau, Tremayne, arrived from Sierra Leone with a cargo of timber, being one of the last ships of the season. She left the colony on the 25th of July, just as the rainy season commenced. The colony was generally healthy. New barrack apartments have been completed for the troops. The trade of the colony had greatly increased. Not less than 20,000 tons of shipping, trading in timber and gum, had left the colony this season. Since the Ashantee war, the traders in gold-dust in the interior had taken a new course to Sago (the capital of Bambarra, about a month's journey from Sierra Leone), thence to Timboo (the capital of the Fulla country), and down to Sierra Leone: the trade is now very considerable by this route. General Turner had returned from his first visit to the settlements on the gold coast, made with a view principally to the extension of trade.

An English establishment has been formed in the island of Mombass, on the east coast of Africa, where a trade in ivory and gum copal is extensively carried on. It appears that Capt. W. F. Owen, of the Leven, who has two surveying ships under his orders, put in there in February 1824 for water, when he found the place under strict blockade by the Imaum of Muscat's vessels. On his landing, the chiefs and principal inhabitants of the place escorted him to the castle, when they solicited from him permission to put themselves under the flag and paternal government of His Majesty George the Fourth; with which request Captain Owen complied (until His Majesty's pleasure should be known), as a measure most likely to conduce to the total suppression of the slave-trade on the coast, where it had been carried on to a most lamentable excess. Lieut. Emery, R.N., with a party of men, was left in command, since which several dows have been captured, the poor slaves released, and the cargoes of the vessels, consisting of grain, cocoa-nuts, and ivory, restored to the owners. The following account of this new establishment (extracted from a private letter just received) must prove acceptable to our readers:—

"Mombassa is an island in 4° 3′ South Lat., and 39° 41′ East Long. about 14 miles in circumference, situate at the mouth of two rivers, distance from the nearest part of the main about two hundred yards; at low water you are able to walk across: it is very fertile and very high. It was at one time in the possession of the Portuguese, who fortified the place

place very strongly; but now the fortifications are going fast to decay, they having been driven out by the Arabs in the year 1720; and I firmly believe that nothing has been done to the battlements since that time. The Arabs are now intermarried with the Sochilles, the native tribe of the place. The harbours are very fine: the chief commerce is ivory and gum copal, which articles are brought into the island by an inland tribe called Whanekas. On the main we have numbers of wild beasts, but none on the island, excepting hyænas: the hippopotami are in great numbers up the rivers."

We have received accounts of a recent discovery in Central Africa, which will soon be laid before the public in greater detail, but of which the following outline is sufficiently curious:—Major Clapperton and Captain Denham, in the course of their late expedition in that quarter of the world, arrived in the territory, and subsequently resided for some weeks, in the capital of a nation, whose manners and history seem likely to occupy, to no trivial extent, the attention of the public of this country—we might safely say of the whole civilized world. They found a nation jet black in colour, but not in our sense of the term *negroes*, having long hair and fine high features. This people was found to be in a state of very high civilization; and above all, the British travellers witnessed a review of seven thousand cavalry, divided into regular regiments, and all clothed in complete armour. Six thousand wore the perfect hauberk mail of the early Norman knights: most strange by far of all, one thousand appeared in perfect *Roman* armour. The conjectures to which this has given rise are various. We confess for ourselves, that, looking to the polished and voluptuous manners ascribed to these people, the elegance of their houses, &c. &c.; in a word, the total difference between them and any other race as yet discovered in the interior of "Africa, the mother of monsters," our own opinion is strongly that here we have a fragment of the old Numidian population,—a specimen of the tribes who, after long contending and long cooperating with imperial Rome, were at last fain to seek safety in the central Desert, upon the dissolution of the empire. In these squadrons Messrs. Clapperton and Denham probably beheld the liveliest image that ever has been witnessed by modern eyes, of the legions of *Jugurtha*—may we not say, of *Hannibal?* The armour, we understand, is fabricated in the most perfect style of the art; and the Roman suits might be mistaken for so many Herculanean or Pompeian discoveries, if it were possible for us to imagine the existence of

of genuine antiques possessing all the glossy finish of yesterday's workmanship.

One of these travellers has already set off on his return to this sable court.—*New Times, Sept.* 27.

LIST OF NEW PATENTS.

To James Butler, of No. 64, Commercial Road, Surrey, for a method of making coffins for the effectual prevention of bodies being removed therefrom.—Dated 12th Aug. 1825.—2 months to enrol specification.

To Joseph Alexander Taylor, of Great St. Helen's, London, for a new polishing apparatus for household purposes.—13th Aug.—6 months.

To Charles Downing, of Bideford, Devon, for improvements in fowling-pieces and other fire-arms.—15th Aug.—2 months.

To Andrew Shoolbred, of Jermyn-street, St. James's, tailor, for his improvements on, or a substitute for, back-stays and braces to prevent relaxation of the muscles.—18th Aug.—6 months.

To George Henry Lyne, of John-street, Blackfriars Road, and Thomas Stainford, of Great Guildford-street, Southwark, engineers, for improvements in machinery for making bricks.—25th Aug.—6 months.

To William Parr, of Union-place, City Road, for improvements in the mode of propelling vessels.—27th Aug.—6 months.

To John Bowler, of Nelson-square, Blackfriars Road, and Thomas Galon, of the Strand, hat-manufacturers, for improvements in the construction or manufacture of hats.—27th Aug.—6 months.

To Charles Mercy, of Edwards Buildings, Stoke Newington, Middlesex, for improvements in propelling vessels.—8th Sept.—2 months.

To William Jefferies, of London-street, Radcliffe Cross, Middlesex, brass-manufacturer, for his machine for impelling power without the aid of fire, water, or air.—15th Sept.—6 months.

To Jean Antoine Teissier, of Tottenham Court Road, Middlesex, for certain improvements, communicated from abroad, in steam-engines.—15th Sept.—6 months.

To Cathcart Dempster, of Lawrence Poultney Hill, London, for his improved cordage.—15th Sept.—6 months.

To George Holworthy Palmer, of the Royal Mint, civil engineer, for his new arrangement of machinery for propelling vessels through the water, to be effected by steam or any other power.—15th Sept.—6 months.

To Adam Eve, of Louth, Lincolnshire, carpet-manufacture, for certain improvements in manufacturing carpets, communicated to him by William Augustus Prince residing abroad, which he intends to denominate "Prince's patent union carpet."—15th Sept.—6 months.

To Isaiah Lukens, late of Philadelphia, but now of Adam-street, Adelphi, machinist, for a surgical instrument for destroying the stone in the bladder without cutting, which he denominates "Lithontriptor."—15th Sept.—6 months.

To Sir Thomas Cochrane, knt. (commonly called Lord Cochrane), of Tonbridge Wells, Kent, for a new method of propelling vessels and boats at sea.—15th Sept.—6 months.

To Charles Jacomb, of Basinghall-street, London, wool-broker, for improvements in the construction of furnaces, stoves, grates, or fire-places.—15th Sept.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. CARY in London, and Mr. VEALL at Boston.*

Gosport, at half-past Eight o'Clock, A.M.								Clouds.							Height of Barometer, in Inches, &c.		Thermometer. London.			Thermometer. Boston.	Rain.		Weather.		
Days of Month, 1825.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	Boston 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Aug. 26	30·13	64	53·85	64	NE.	...	0·490	1	1	1	...	1	1	1	30·19	29·76	59	70	57	62	...	...	Fair	Fine	NW.
27	30·04	60	.....	96	NE.	...	·485	...	...	1	...	...	1	1	30·07	29·73	60	60	56	61	...	...	Rain	Cloudy	W.
○ 28	29·98	59	.....	84	N.	0·25	.....	...	...	...	...	...	1	1	30·05	29·62	58	65	60	60	0·45	...	Cloudy	Cloudy	NE.
29	30·09	64	.....	85	SW.	...	·110	...	1	1	1	...	1	1	30·08	29·60	60	66	66	61	...	0·09	Rain	Rain	SE.
30	30·12	68	.....	77	W.	...	.....	...	...	1	1	1	...	...	30·14	29·60	66	76	69	70	...	...	Fair	Fine	SE.
31	30·12	67	.....	71	NE.	·30	.....	1	1	1	...	1	1	...	30·14	29·60	68	76	64	68	...	...	Fair	Cloudy	SE.
Sept. 1	30·09	68	54·05	64	N.	...	.....	...	...	1	...	1	...	...	30·15	29·60	66	76	66	70	...	...	Fair	Fine	SW.
2	30·19	70	.....	58	NE.	...	.....	1	1	...	1	1	1	...	30·22	29·70	66	71	63	63	...	...	Fair	Fine	NE.
3	30·20	64	.....	65	E.	·70	.....	...	1	...	...	1	1	...	30·22	29·73	58	68	56	63·5	...	...	Fair	Fine	NW.
4	30·06	60	.....	60	N.	...	.....	1	...	1	...	1	...	...	30·04	29·62	56	65	50	58·5	·20	...	Fair	Fine	NW.
☾ 5	30·09	55	.....	58	N.	...	.....	...	...	1	...	1	1	...	30·09	29·68	54	60	49	56	...	...	Cloudy	Cloudy	NW.
6	30·02	54	54·05	61	NW.	·50	.....	1	...	1	...	...	1	...	29·95	29·60	54	64	57	59·5	...	...	Cloudy	Cloudy	NW.
7	29·82	58	.....	58	NW.	...	·030	1	1	1	...	1	1	1	29·77	29·37	57	65	55	58	...	...	Fair	Fine	W.
8	29·65	61	.....	64	NW.	...	.....	1	1	1	...	1	1	...	29·69	29·20	56	67	57	60	...	...	Fair	Fine, rain at night	W.
9	29·77	62	.....	62	SW.	·65	.....	1	...	1	1	1	1	...	29·76	29·30	56	68	59	58	...	·27	Fair	Fine	SW.
10	29·66	66	.....	63	S.	...	·500	1	1	1	...	1	1	1	29·65	29·25	58	68	60	62·5	...	...	Rain	Fine, rain at night	N.
11	29·57	63	.....	67	S.	...	·010	1	...	1	...	1	1	1	29·64	29·14	60	67	60	64	·25	·28	Cloudy	Fine	S.
● 12	29·72	66	.....	68	S.	·55	·010	1	1	1	...	1	...	1	29·87	29·30	61	67	60	65	...	...	Fair	Fine	SE.
13	29·70	66	54·40	70	E.	...	·065	1	...	1	...	1	1	1	29·76	29·34	66	67	60	67·5	...	...	Showery	Fine	E.
14	29·46	64	.....	73	NE.	...	·025	...	...	1	...	...	1	1	29·44	29·10	60	68	60	63	...	...	Fair	Cloudy	NW.
15	29·56	63	.....	73	NW.	·25	·050	1	...	1	...	1	1	1	29·70	29·10	61	66	61	63·5	...	...	Cloudy	Cloudy	NE.
16	29·82	66	.....	77	S.	...	·490	...	...	1	...	...	1	1	29·86	29·38	62	68	64	61·5	...	...	Fair	Cloudy [ring the day	SW.
17	29·83	66	.....	85	S.	...	·205	1	1	1	...	1	1	1	29·85	29·33	66	68	62	65·5	...	·07	Showery	Cloudy, showers du-	S.
18	29·84	65	.....	72	SW.	·30	.150	...	...	...	...	...	1	1	29·85	29·35	63	67	64	62	1·45	·16	Cloudy	Fine, rain at night	SW.
19	29·81	66	.....	82	SW.	...	.....	1	1	1	...	1	1	...	29·85	29·23	66	69	66	66·5	...	...	Cloudy	Rain	SW.
☽ 20	29·80	64	55·00	78	SW.	...	·100	1	1	1	...	...	1	1	29·82	29·26	64	69	64	63	...	...	Fair	Cloudy, rain at night	SW.
21	29·60	65	.....	86	S.	·35	·040	1	1	1	...	1	1	1	29·62	29·08	64	67	58	66	...	·04	Showery	Cloudy, rain *p.m.*	SW.
22	29·69	63	.....	71	W.	...	.....	1	...	1	...	1	1	...	29·80	29·13	58	65	55	61	...	·09	Cloudy	Fine	NW.
23	30·02	61	.....	70	E.	...	·005	1	...	1	...	1	1	1	30·03	29·66	51	64	60	51·5	...	...	Fair	Fine	NW.
24	30·08	66	.....	71	SW.	...	·005	1	...	1	...	...	1	1	30·10	29·53	60	66	64	64	...	...	Cloudy	Cloudy	SW.

THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL.

31st OCTOBER 1825.

XXXIII. *On the Constitution of the Atmosphere.* *By* J. IVORY, *Esq. M.A. F.R.S.*

[Continued from p. 93.]

5. IT appears from what has been said in this Journal for August last, that when a mass of air rises upwards in the atmosphere, the heat absorbed by the rarefaction is greater than the loss of temperature. In the course of its ascent it receives heat from the contiguous fluid, which in part balances what disappears on account of the continual dilatation. But although the loss of temperature is thus reduced below the heat that enters into combination in a latent form; yet both these quantities increase together, and hence arises the degree of cold that is found to prevail in ascending above the earth's surface.

The heat absorbed in rising to any height x, and the whole extraneous heat received, being denoted as before by i and θ, and the loss of temperature by t, we shall have

$$i - \theta = t:$$

And this value being substituted in the equations (F), we shall get

$$p = \left(\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}\right)^3 \times \frac{1+\alpha\tau-\alpha t}{1+\alpha\tau},$$

$$\varrho = \left(\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}\right)^3.$$

In these equations τ is the temperature at the earth's surface, ϱ the density and p the elasticity at the height x. The atmosphere being *in equilibrio*, the pressure is equal to p; and the quantities p, i, t vary together, since they all depend upon x. We therefore get by taking the fluxions,

$$-\frac{dp}{\varrho} = \frac{\alpha\, dt}{1+\alpha\tau} \cdot \left\{3\,\frac{di}{dt} \cdot \frac{1+\alpha\tau-\alpha t}{1+\alpha\tau-\alpha i} + 1\right\}.$$

But we likewise obtain from the equation (H),

$$-\frac{dp}{\varrho} = ds = \frac{dx}{l(1+\alpha\tau)}.$$

Wherefore by equating the equal quantities,

$$\frac{dx}{dt} = \alpha l \times \left\{ 3 \frac{di}{dt} \cdot \frac{1 + \alpha\tau - \alpha t}{1 + \alpha\tau - \alpha i} + 1 \right\}. \qquad \text{(K)}$$

In the equation just obtained if we suppose $i = t$, which is the hypothesis of Dalton, then,

$$\frac{dx}{dt} = 4\alpha l;$$

or, since $\alpha = \frac{3}{800}$, and $l = 4500$ fathoms at the mean temperature of 50° Fahrenheit,

$$\frac{dx}{dt} = 67\tfrac{1}{2} \text{ fathoms.}$$

It thus appears that in the atmosphere of Dalton, the temperature decreases exactly at the same rate as the height increases, the depression of the mercury in the thermometer being one centesimal degree for every $67\frac{1}{2}$ fathoms of ascent. The total altitude is equal to $4l$, or about 20 miles; for at that height the density is evanescent.

But the like properties belong to an indefinite class of atmospheres. Assume

$$m = 3 \frac{di}{dt} \cdot \frac{1 + \alpha\tau - \alpha t}{1 + \alpha\tau - \alpha i};$$

then

$$\frac{1 + \alpha\tau - \alpha t}{1 + \alpha\tau} = \left(\frac{1 + \alpha\tau - \alpha i}{1 + \alpha\tau} \right)^{\frac{3}{m}} = \varrho^{\frac{1}{m}};$$

and consequently

$$p = \varrho^{1 + \frac{1}{m}}$$

$$\frac{dx}{dt} = \alpha l (m + 1).$$

The essential character of all these atmospheres, including every case in which the height is proportional to the temperature lost in ascending, is marked by a single equation between the pressure and density, viz.

$$p = \varrho^{1 + \frac{1}{m}};$$

and the relation between the altitude and the loss of temperature is expressed by this formula, viz.

$$x = \alpha l (m + 1) t = \frac{135 (m + 1)}{8} \cdot t.$$

This class of atmospheres has been found in a different way in the paper in the Philosophical Transactions for 1823, already cited. The reasoning in that paper proceeds solely upon the received laws of the equilibrium of elastic fluids; no hypothesis being admitted except the particular condition the consequences of which it is proposed to investigate, namely, that the height ascended shall be proportional to the decrease of temperature. In this manner of considering the subject there

there is no restriction to the arbitrary number m, which may vary from zero to infinity. But it has here been attempted to deduce the laws of elastic fluids, established independently by experiments, from the relations that subsist between the variations of the bulk of a mass of air, the heat that enters into combination in a latent form, and the heat of temperature sensible to the thermometer. From this view of the subject it follows that m cannot be less than 3; for this is its value in the atmosphere imagined by Dalton, in which air of a given density possesses the greatest possible degree of cold that can be produced by rarefaction. If we suppose that m is less than 3, we contemplate an atmosphere in which the density, the pressure and the temperature cannot subsist together according to the laws observed in nature. But m may have any value greater than 3. Although the cold in the atmosphere cannot be supposed to pass the limit imposed by nature, it may fall short of that limit by means of heat transferred from the contiguous fluid or received from other sources. Experience shows that this is the fact; for the cold at any height is less than what would be produced by dilating the air to the degree that prevails at that height. The value of m varies above 3, according as we suppose that a less or greater portion of the heat of combination is restored to the rarefied air from extraneous sources; and it becomes infinitely great when, the whole heat of combination being supplied, the same temperature prevails uniformly in every part of the atmosphere. So long as m has a finite value, the total height of the atmosphere is limited, and equal to $l \times (m + 1)$, or to $5\,(m + 1)$ in miles. But in the atmosphere of equable temperature, when m is infinitely great, the height is unlimited.

In order to find the particular atmosphere which, in the class we are considering, approaches nearest to that of nature, we must determine m so as to make the gradation of heat agree with what is actually observed at the earth's surface. Allowing that the centigrade thermometer is depressed one degree for every 90 fathoms of ascent, we shall therefore have

$$90 = \frac{135}{8} \times (m + 1),$$

$$m = \frac{13}{3}.$$

In round numbers m is thus equal to 4; and the pressure and temperature will be expressed in terms of the density by these equations, viz.

$$p = \varrho^{\frac{5}{4}}$$

$$\frac{1 + \alpha\tau - \alpha t}{1 + \alpha\tau} = \varrho^{\frac{1}{4}}. \qquad \text{(a)}$$

In order to form a just idea of the theory we must now compare these equations with observation. By combining them we get,

$$p = \left(\frac{1 + \alpha\tau - \alpha t}{1 + \alpha\tau}\right)^5.$$

As τ is the temperature at the earth's surface, and t the temperature lost in ascending, if we put $\tau' = \tau - t$, then τ' will be the actual temperature in the atmosphere; and we shall get

$$\log.\ \frac{1}{p} \div \log.\ \frac{1 + \alpha\tau}{1 + \alpha\tau'} = 5. \qquad \text{(b)}$$

Supposing that the centigrade thermometer is used, α will be $\frac{3}{800}$, and the last formula will become

$$\log.\ \frac{1}{p} \div \log.\ \frac{800 + 3\tau}{800 + 3\tau'} = 5,$$

which we may apply to the measurements recorded in p. 189 of Ramond's work. The dividends, or the logarithms of $\frac{1}{p}$, are there found ready calculated, but only to 3 places of figures. The quantities τ and τ' are also given in separate columns, and from these the divisors must be computed. In this manner the following table has been constructed, in which only the first figure of the quotient is set down, as the observations do not comport with a greater degree of accuracy.

Places.	Heights.	τ	τ'	Dividends.	Divisors.	Quotients.
	yds.	°	°			
Gay-Lussac's ascent	7630	+ 30·8	— 9·5	0·363	·063	6
Chimborazo	6427	25·3	— 1·6	0·304	·042	7
Montblanc, Geneva	4782	28·3	— 2·9	0·229	·049	5
———	—	27·6	— 1·6	0·229	·046	5
Pic de Teneriffe . .	4077	24·9	+ 8·4	0·190	·026	7
Montblanc, Chamouny	4070	23·0	— 2·9	0·197	·041	5
———	—	25·0	— 1·6	0·196	·042	5
Etna	3540	23·1	+ 4·4	0·172	·029	6
Montperdu, Tarbes	3408	25·6	6·9	0·155	·029	5
Col du Géant, Geneva	3346	24·9	4·5	0·153	·032	5
Maladette	3174	20·8	3·4	0·150	·027	5
Pic du Midi, Tarbes	2858	27·5	11·6	0·131	·024	$5\frac{1}{2}$
———	—	19·6	8·6	0·133	·017	8
———	—	22·5	8·1	0·135	·022	6
———	—	23·5	10·4	0·133	·020	$6\frac{1}{2}$
———	—	18·8	8·1	0·135	·017	8
———	—	19·1	4·0	0·136	·023	6
———	—	14·8	4·3	0·134	·016	8

Places.	Heights.	τ	τ'	Dividends.	Divisors.	Quotients.
	yds.	°	°			
Col du Géant, Chamouny	2606	+21·6	+ 4·5	0·121	·026	5
Montperdu, Bareges	2354	25·0	6·9	0·111	·028	4
Pic d'Eyré, Tarbes	2347	21·3	11·0	0·109	·016	7
Pic de Montaign . .	2244	14·5	3·1	0·108	·018	6
Pic du Midi, Bareges	1808	26·7	16·4	0·084	·016	5
———	—	21·9	8·0	0·085	·022	4
———	—	21·3	8·2	0·085	·020	4
———	—	18·5	6·0	0·086	·019	$4\frac{1}{2}$
———	—	15·9	2·5	0·087	·022	4
———	—	17·8	7·0	0·086	·017	5
———	—	18·9	5·8	0·086	·021	4
———	—	18·4	5·2	0·086	·020	4
Puy de Dome, Clermont	1163	21·3	14·4	0·054	·011	5
———	—	17·8	10·8	0·055	·011	5
———	—	18·6	11·7	0·055	·011	5
———	—	24·8	15·2	0·054	·014	4
———	—	32·9	23·4	0·052	·014	4
Bédat du Bagnères, Tarbes	611	10·9	8·0	0·029	·005	6
Pont du Berges, Clermont	537	0·3	— 2·9	0·026	·005	5
La Barrague, Do. .	415	23·6	+21·8	0·019	·003	6

The inspection of this table shows that the theory agrees with observation in 16 instances out of 38; in 7 instances, the results of observation are considerably above the theory; and in 15, the differences are not greater than might be expected. The deviations from the theory seem to be accidental; they are indiscriminately in excess and in defect at all heights, and in measuring the same height at different times. The mean of the whole is 5·4, agreeing with $5\frac{1}{3}$ which is the number determined by the theory.

With regard to the quantities found by observation, no objection can be made to the accuracy of the numbers derived from the barometer. But the case is different with regard to the thermometer. That instrument, it is well known, is affected in such experiments by many local and accidental causes, which render its determinations irregular and uncertain, but which it would be superfluous to consider here. It is only necessary

necessary to remark, that in many of the instances the difference of temperature is inconsiderable, and that a variation of a degree or two at either extremity of the height measured will materially change the divisor, and occasion a great uncertainty in the quotient.

If we expand the logarithms in the divisor of the formula (b), and reject the powers above the square, we shall get

$$\log.\frac{1+\alpha\tau}{1+\alpha\tau'} = \alpha(\tau-\tau')\times\left\{1-\frac{\alpha(\tau+\tau')}{2}\right\} = \frac{\alpha(\tau-\tau')}{1+\alpha.\frac{\tau+\tau'}{2}}.$$

Wherefore, by substitution,

$$\frac{\log.\frac{1}{p}}{\alpha(\tau-\tau')}\times\left\{1+\alpha.\frac{\tau+\tau'}{2}\right\} = 5.$$

But, from the relation that subsists between the difference of temperature and the height in the class of atmospheres we are considering, we have $\alpha l \times 5 \times (\tau-\tau') = x$, and

$$\frac{x}{\alpha l(\tau-\tau')} = 5.$$

Hence, by equating the equal quantities, we obtain

$$x = l \times \log.\frac{1}{p} \times \left\{1+\alpha.\frac{\tau-\tau'}{2}\right\}, \qquad (c)$$

which is no other than the usual formula for finding any height in the atmosphere by means of the barometer.

It has now been proved that air of a given elasticity, or subjected to a given pressure, has the same temperature in the atmosphere we are considering as in that of nature; and the last formula proves that the same pressure likewise takes place in both cases at the same altitude. It appears therefore that the equations (a) represent the real state of the earth's atmosphere as nearly as the experimental knowledge in our possession enables us to judge.

It deserves to be mentioned, that in the investigation of the barometrical formula (c), the particular number 5 disappears, and the result is therefore true for all values of the general symbol m. That formula is a general property of all the atmospheres in which the loss of temperature is proportional to the height, whatever be the rate of the gradation of heat. But it likewise belongs to many other atmospheres, besides the class mentioned; and we may affirm generally, that the usual barometrical formula is an approximation independent of any particular law of the gradation of heat, and that it is true in every atmosphere in which the known laws of elastic fluids are supposed to prevail. If we consider that in practice τ and τ' are found by observation, and that these quantities affect the height measured only in a small degree, it will be admitted that

that the heights in the atmosphere are ascertained with much greater exactness than the temperatures, which are subject to so many causes of irregularity. The great obstruction to our knowledge of the constitution of the atmosphere is the want of thermometrical experiments of a sufficient degree of exactness, by means of which to correct and guide the determinations of theory.

Although the atmosphere determined by the equations (a) agrees with nature as far as the observations in our possession enable us to judge, yet this is proved only with regard to altitudes that do not pass a certain limit. The total height does not exceed 25 miles, which is probably less than half the extent of the earth's atmosphere. Allowing therefore that the two atmospheres coincide in their lower parts, yet they must ultimately diverge greatly from one another. If instead of taking $m = 4$, we make it 5, which is a supposition not inconsistent with observation, the objection will still remain, as the total height would be increased only to 30 miles. This then is the objection to an atmosphere in which the decrease of heat is proportional to the height, that the total altitude does not quadrate with experience. The gradation of heat that obtains in nature must therefore follow a different law. According to what has already been observed, there can be no doubt that, as we ascend higher, the height requisite to depress the thermometer one degree continually increases, although the irregularity of the observations is so great as to render it impossible to ascertain the rate of increase with any tolerable precision. To illustrate this point, let us again have recourse to the experiments recorded by Ramond, which may be partitioned in two classes; one containing 16, in which the height is less than 2000 yards; and the other 22 of greater altitude. The mean height for depressing the centigrade thermometer one degree deduced from the first class is 144 metres or 78 fathoms; and the same height in the other class is 180 metres, or 98 fathoms. The difference of these two determinations is considerable, and proves incontestibly a retardation of the decrease of heat in ascending.

As we have it not in our power to determine upon sure principles the relation that subsists between i and t in the atmosphere, we can only make probable conjectures concerning it. The most simple supposition is, that the two quantities have always the same proportion to one another. Now, t being less than i, let us put

$$t = (1 - \beta)\, i\,;$$

then,

then, $$\frac{1+\alpha\tau-\alpha t}{1+\alpha\tau} = \beta + (1-\beta).\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}:$$

and hence, observing that $\tau' = \tau - t$, we get,

$$p = \beta\left(\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}\right)^3 + (1-\beta)\left(\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}\right)^4 \qquad \text{(d)}$$

$$\frac{1+\alpha\tau'}{1+\alpha\tau} = \beta + (1-\beta).\frac{1+\alpha\tau-\alpha i}{1+\alpha\tau}.$$

If we suppose $\beta = 0$, the equations just found will be the same as in the atmosphere of Dalton; and if $\beta = 1$, they will belong to the atmosphere of equable temperature. When β has any mean value, the equation will determine an atmosphere intermediate between the two extreme cases, and participating of the nature of both.

To find the numerical value of β, we must employ the equation (K). By the proper substitutions, we shall get,

$$\frac{dx}{dt} = \alpha l \times \left\{4 + \frac{3\beta}{1-\beta} \times \frac{1+\alpha\tau}{1+\alpha\tau-\alpha i}\right\}.$$

The inspection of this formula shows that $\frac{dx}{dt}$ increases when i increases; that is, the decrease of heat becomes slower the higher we ascend in the atmosphere. At the surface of the earth we have $\frac{dx}{dt} = 90$, and $i = 0$, and hence $\beta = \frac{4}{13} = \frac{3}{10}$ very nearly.

We may now compare the gradation of heat in this atmosphere with the calculations already made. For this purpose put $\frac{\alpha i}{1+\alpha\tau} = u$, $f = 1-\beta = \frac{7}{10}$: then

$$p = (1-u)^3 \times (1-fu)$$

$$\frac{1+\alpha\tau'}{1+\alpha\tau} = 1-fu. \quad \text{Consequently}$$

$$\log.\frac{1}{p} \div \log.\frac{1+\alpha\tau}{1+\alpha\tau'} = 1 + 3\left\{\log.\frac{1}{1-u} \div \log.\frac{1}{1-fu}\right\}.$$

From these expressions we readily obtain this formula, which is a sufficient approximation, viz.

$$\log.\frac{1}{p} \div \log.\frac{1+\alpha\tau}{1+\alpha\tau'} = \frac{3+f}{f} + \frac{3(1-f)}{2f(3+f)}.\log.\frac{1}{p};$$

or in numbers,

$$\log.\frac{1}{p} \div \log.\frac{1+\alpha\tau}{1+\alpha\tau'} = 5\frac{2}{7} + \frac{45}{259}\log.\frac{1}{p}.$$

In all accessible heights the last term on the right-hand side of the formula is inconsiderable, and may be neglected; and the calculations already made prove that the gradation of heat in

in this atmosphere will agree with the mean result deduced from the experiments recorded by Ramond.

We may now generalize the conclusion at which we have just arrived.—Every atmosphere in which the laws of elastic fluids are observed, and in which the initial gradation of heat is made to coincide with that which obtains at the earth's surface, will represent terrestrial experiments taken at a mean with sufficient accuracy. Unless we can push our experiments to heights in the atmosphere hitherto inaccessible, or remove the accidental irregularities of the thermometrical experiments in that part of it within our reach, it may be said that the present research is brought to a conclusion. Many atmospheres may be found in which the pressure, the temperature, and the density, shall agree with nature in a mean of many experiments.

The same atmospheres likewise represent the astronomical refractions with considerable accuracy. In this respect they agree with one another and with nature, unless at very low altitudes. Even the horizontal refractions are not much different in the several cases, and approach nearly to the quantity found by observation,—if indeed we can affirm any thing of an element hitherto ascertained with so little precision. But the atmospheres vary more from one another in the celestial, than in the terrestrial, phænomena. In the one case, the light of a star traverses the whole of the earth's atmosphere; in the other, our experiments are confined to a very limited altitude. It is sufficient for the terrestrial phænomena, that an atmosphere coincide with that of nature in its lower part; but the astronomical refractions require that both coincide nearly in their whole extent.

Of all the atmospheres which agree with terrestrial experiments, that determined by the equations (a), in which the gradation of heat is equable, must diverge soonest, and in the greatest degree, from the earth's atmosphere in which the height requisite for depressing the thermometer a given quantity continually increases in ascending. The atmosphere determined by the equations (d) is preferable; because the rate of the decrease of heat becoming gradually slower, the coincidence with nature must reach to a greater extent. In reality, if the refractions computed from the equation (a) and (d) be compared with the quantities determined by observation, although there will be found almost an exact coincidence to a great distance from the zenith, yet the errors of the former become considerable sooner, or at greater altitudes, than those of the latter.

In the equations (d), if the pressure be expressed by means of the density $(1-\omega)$, we shall get,

$$p = \beta(1-\omega) + (1-\beta)(1-\omega)^{\frac{4}{3}} \qquad \text{(e)}$$

In the Philosophical Transactions for 1823, the refractions are computed by means of this equation, viz.

$$p = (1-f)(1-\omega) + f(1-\omega)^2,$$

f being equal to $\frac{1}{4}$. Now if we determine β so as to make the initial gradation of heat the same in the two atmospheres, the refractions in both will very nearly coincide, the difference amounting only to a few seconds at the horizon. And although the matter is not of much importance, yet it seems reasonable to give the preference to the formula (e), which is a mean between two extreme cases pointed out by the laws to which air is subject in its dilatations.

Instead of determinating β by means of the gradation of heat at the earth's surface, in which there is considerable certainty, it will be better to employ for the same purpose some good observed refractions. About eight or ten good observations will be required between the altitudes 0° and 10°; for at greater altitudes the refractions are nearly the same in every constitution of the atmosphere. We must then compute the same refractions in the two atmospheres of the formula; and β will be found by making each observation the same mean between the quantities computed in the two extreme cases. If we make $\beta = \frac{1}{4}$, the horizontal refraction will be about 34′, and the height for depressing the centesimal thermometer one degree equal to 84 fathoms, supposing the standard barometer 30 in. and the mean temperature 50° Fahrenheit; and if $\beta = \frac{1}{2}$, the horizontal refraction will be about 35′, and the gradation of heat 120 fathoms for one centesimal degree. The true atmosphere is undoubtedly contained between these limits.

Oct. 6, 1825. JAMES IVORY.

XXXIV. *Remarks* on the Method proposed by Mr.* BURNS *for finding the Latitude by Double Altitudes. By* E. RIDDLE, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

MY attention has been drawn to a communication in your last Number, from James Burns, B. A.; and I trouble you with a few lines, to point out that Mr. Burns has *not* done what he imagines he has done; viz. *given an improved solution to the double altitude problem.*

[* See also a letter from Thomas Henderson, Esq. *infra* p. 283.—EDIT.]

Indeed,

Indeed, Mr. B. altogether misapprehends the nature of the problem: for he assumes, as known, not only the interval of time between the observations, but *the true apparent time at each observation, both* of which data, he says, may be obtained with the greatest exactness from the improved state of chronometers.

Now it may be observed on this,—that if the apparent time at *either* observation were known, the other observation and time would be superfluous data; as the problem would then, without their aid, become one of the simplest in nautical astronomy. But it is not, as Mr. B. has incautiously inferred, the object of chronometers to point out the time *at any place at which an observation may be made;* but to show, at any instant, what the time is at some individual meridian: and therefore, though the *interval* in mean time may be correctly obtained from a chronometer, the actual times as shown by a chronometer may differ in any way from the *apparent times at the place of observation;*—it is sufficient for the computer, if he know the Greenwich time nearly enough to take out the declination with requisite correctness. The times A.M. and P.M. in the examples which Mr. B. has re-computed are by no means intended to represent the true apparent times of observation; their only use is to determine the elapsed interval, and to find, with the aid of the estimated longitude, the approximate Greenwich time for determining the declination.

In fact, Mr. Burns has proposed to himself the solution of one problem; and he has, unknowingly, given the solution of another,—and of one for which, in nautical practice, the necessary data cannot be obtained.

I have only to add, that I believe the simplest solution of this useful problem that has yet been given, was deduced by myself from formulæ that were first investigated by Mr. Ivory. This solution was first published in the Philosophical Magazine, and has been copied into other works in general use among nautical men. It is *direct*, *accurate*, and perhaps as concise as from the nature of the problem any solution of it can be expected to be. Mr. Burns's proposed solution, as we have seen, is not a solution to the problem at all.

The true answers to the examples which Mr. Burns has recomputed, are 27° 59′ 16″, and 9° 59′ 32″.

The same answers by Dr. Brinkley's approximate method of solution, are 27° 59′, and 10° 1′.

Mr. Burns makes the answers 27° 0′, and 8° 58′; and he notices the discrepancies between his results and Dr. Brinkley's with a mark of admiration. The difference is certainly

 remarkable;

remarkable; but I have no doubt that Mr. Burns will now regret that he has been so hasty in attributing the discordance to the principles of Dr. Brinkley's solution.

I am, sir, yours, &c.

Greenwich Hospital, Oct. 6, 1825. E. Riddle.

XXXV. *Observations on the Tests for Arsenic*.* By Aug. Ludw. Giseke.

1. *On the Detection of Arsenic by Lime-water.*

I GIVE, by way of introduction, a short account of some experiments on the subject, made some time since at this university, and which were occasioned by the following passage in Berzelius's excellent Instructions in Chemistry (*Lehrbuche der Chemie*). Berzelius mentions in the second volume of this work, p. 152 (according to Palmstedt's translation), the chemical detection of poisoning by arsenic, for which he gives the following process as proposed by Rose, with some additions of his own: "Cut up the coats of the stomach, and place them in the liquid, which is boiled with a few drachms of caustic potash, in order to dissolve any arsenious acid that might be contained in it. The solution obtained is filtered, heated till it boils, and during the boiling mixed with nitric acid, which is added in small portions as long as any thing separates, and till the liquid has become strongly acid, clear, and of a bright yellow colour. It is filtered while hot, afterwards nearly, not completely, saturated with carbonate of potash, and made to boil, in order to expel the carbonic acid; then it is boiled with clear lime-water as long as a precipitate is formed. The lime-water first saturates the excess of acid, and then precipitates with the arsenious acid as arsenite of lime, and with the phosphoric acid and other animal substances decomposed in the nitric acid. If instead of saturating the acid with lime-water, you add first caustic alkali, till the liquid becomes alkaline, and then add lime-water, no precipitate will be formed, because the arsenite of lime is held in solution by the alkali."

It has been already observed by Hahnemann, that the arsenite of lime is dissolved even by the weakest acid of any kind; but no one noticed before Berzelius that it was also soluble in a saturated alkaline solution; and it also contradicted the experiments made by Prof. Schweigger in his chemical lectures,

* From Schweigger's *Journal*, Band xiii. p. 359.—This article is condensed from several lectures given by the author at the Physical Seminary at Halle.

which

which showed that arsenite of lime could not be dissolved even by a great excess of alkali. In these experiments, to liquids poisoned with arsenic, caustic potash as well as soda and ammonia were purposely added; and immediately on adding lime-water, a very copious precipitate of arsenite of lime was produced.

Prof. S. considered this subject the more worthy of being investigated, as it surprised him that so distinguished a chemist as Berzelius,—who is so justly respected as the first analyst of our age,—should be opposed to nature in so simple a process. Besides, the rule laid down by this careful naturalist had been already pretty generally admitted. Thus Geiger, in his new and valuable *Handbuche der Pharmacie*, p. 431, says "that the precipitate produced by lime-water in a pure aqueous solution of arsenious acid might be dissolved by *free acids* or *alkalis*." And Buchner, in his *Toxikologie*, p. 405: "Lime-water will produce a white precipitate as well in arsenious as in arsenic acid. In order to produce this effect, neither any free acid nor free lime-water must be in it." In reality there may be a great excess of lime-water, without the result being disturbed by it; on the contrary, it is beneficial to apply lime-water abundantly: on which account Fischer justly recommends rather to put the solution of arsenic into lime-water, than the reverse, because the lime-arsenic is dissolved even by an excess of arsenious acid. But since neither arsenite nor arseniate of lime are dissolved, with any predominance of potash, soda, or ammonia; how was it that Berzelius obtained no precipitate with alkali in excess? The following experiment made by Prof. Schweigger in his lectures on chemistry will explain it.

Prepare an arsenical liquid, pour it into three glasses, and add to one portion an excess of caustic potash; to the second, excess of caustic soda; and to the third, excess of caustic ammonia. On adding lime-water, a deposit of arsenite of lime will be equally formed in each of the glasses. Now add to each a few drops of acid, (for instance, nitric acid,) yet so that in all the alkali shall predominate. Whilst no solution of the precipitate will take place in the glasses that have the potash and soda in them, it will immediately begin in that with the ammonia: and all the arsenite of lime will be finally dissolved, although the ammonia be not saturated by the acid which has been added. Of course the solution will take place in the three glasses when any acid is in excess; yet on saturating the acid with alkali, the precipitate will be re-formed immediately in those glasses that contain the potash or soda, but not

not in that which holds the ammonia, however one may neutralize the liquid. The experiment may also be made by putting sal ammoniac or nitrate of ammonia into a liquid containing arsenic, and adding lime-water, which then will not give a precipitate, however great the quantity of lime-water that may be added; or the reverse, by pouring the arsenical solution into lime-water, and even heating the solution in order to concentrate it more. Thus it will be seen that in the experiment mentioned by Berzelius, and as he has observed himself, no precipitate will be obtained by neutralizing the nitric solution with ammonia, of which he probably made use. But it is not the ammoniac as *caustic alkali*, but the *nitrate of ammonia* which is thereby formed, that prevents the deposit; and if instead of the ammonia we employ caustic potash or soda for neutralizing the nitric acid, the precipitate of arsenite of lime will instantly be formed with lime-water, even with a prevalence of the alkali.

The results of these experiments lead us also to make a few remarks on the method of discovering arsenic in a poisoned liquid, proposed by M. Pfaff in his Manual of Analytic Chemistry. He says (vol. ii. p. 392), "If the white arsenic cannot be separated from the contents of the stomach, the whole substance must be boiled in a proportionate quantity of water and nitric acid. The liquid, which in consequence turns yellow, is filtered through a woollen cloth, and the clarified acidulous liquid mixed with acidulated solution of sulphuretted hydrogen as long as it continues to produce a lemon-coloured turbidity. One may also use *lime-water* instead of the sulphuretted hydrogen, and submit the arsenite of lime to sublimation with 1-4th of charcoal powder." Nevertheless it is evident, from the experiments above recited, that lime-water will not produce a precipitate of arsenite of lime, owing to the ammoniacal salt prevailing in the arsenical liquid.

Roloff likewise proposes in these kinds of experiments to neutralize the nitric acid with solution of caustic potash or ammonia*. If he has made a frequent use of the latter, it may be easily conceived why, deviating from Rose's plan, he has preferred to recommend sulphuretted hydrogen instead of lime-water, since the latter will give no result.

As the effect of an ammoniacal salt here observed, which is so similar to that observed by Mr. Gærtner† in arsenite of copper (Scheele's green), probably depends on double affinity,

* Vide *Journ. f. Chem. u. Phys.* vol. vii. (old series) p. 415.
† *Ibid.* p. 426.

in

—in which, however, we must suppose a much stronger affinity between the arsenic and the ammonia than between the arsenic and the potash or soda,—I have been requested by Professor Schweigger to make a series of experiments on the decomposition of arsenite of lime by the salts of ammonia. The following are the results.

A. I precipitated some arsenious acid with lime-water, and added to this liquid, in which the arsenite of lime was suspended in white flakes, the following ammoniacal salts in solution; viz. the muriate, nitrate, acetate, and sulphate. Each of these produced a solution of the arsenite of lime and clarified the liquid. But when I added to this liquid containing the arsenite of lime in flakes, phosphate or carbonate of ammonia, an effect was produced on the lime, but a deposit always remained. This shows evidently the existence of a double affinity, since the phosphoric and carbonic acids became precipitated in combination with the lime, but with the other ammoniacal salts the solution remains clear, as may be seen from the following experiments.

a. The precipitate formed by carbonate of ammonia effervesced in acetic acid, and diluted with water gave with oxalic acid a white precipitate of oxalate of lime.

b. By adding sulphuretted hydrogen to the clear supernatant liquid a yellow tinge was produced; with nitrate of silver a yellow, and with lime-water a white precipitate.

c. The solution above the precipitate produced by phosphate of ammonia, gave with sulphuretted hydrogen a yellow tinge; with nitrate of silver a yellow, and with lime-water a white precipitate.

B. Arsenite of lime newly precipitated was filtered off, and washed with distilled water: this pappy substance was then put into solutions of acetate, muriate, nitrate, and sulphate of ammonia, and completely dissolved; but in phosphate or carbonate of ammonia a precipitate remained, as described above.

C. The arsenite of lime being well dried was not dissolved by nitrate, acetate, or muriate of ammonia, at common temperatures; but when boiled the solution became quite clear. But if boiled with natural phosphate of ammonia, a precipitate of phosphate of lime remained.

2. *On the Detection of Arsenic by means of Iodine-starch.*

Brugnatelli was the first who stated that iodine-starch might be applied as a re-agent for arsenious acid. His essay on the subject may be found in the *Journ. f. Chem. u. Phys.* vol. xx. p. 56. According to him, iodine-starch is deprived of colour by arsenious acid and by solution of sublimate; but the blue

blue colour is restored by the addition of concentrated sulphuric acid when the experiment has been made with arsenious acid, but not when sublimate has been used. Thus iodine-starch will serve as a true criterion to distinguish between those two kinds of poison.

Being one day present with my friend Moritz Hecker, at a judicial investigation of an alleged poisoning by arsenic, and the absence of both arsenic and sublimate having been ascertained by the acknowledged re-agents, we also tried iodine-starch. The liquid under examination having completely deprived it of colour, we were led to make a series of experiments on the decolorization of iodine-starch and the restoration of its colour. They completely confirmed what had already been affirmed by Stromeyer in Gilbert's *Annalen*, vol. xlix. p. 150, and by Colin and Gaultier de Claubry (*Journ.* vol. xiii. old series, p. 453—457); and nothing can be added, according to our experiments, to the substances mentioned by these chemists as decolorizing the iodine-starch, except prussic acid and prussiate of mercury. That protonitrate of mercury as well as sublimate would deprive it of colour might have been expected. But it deserves to be noticed, that whilst arsenious acid quickly decolorizes the iodine-starch, pure arsenic acid does not, although arseniate of potash and arseniate of ammonia render it colourless; after which, an addition of sulphuric acid restores the colour. The fuming sulphuric acid of Nordhausen, concentrated or somewhat diluted, completely decolorizes iodine-starch; wherefore the blue colour, if destroyed by arsenious acid, cannot be restored by the addition of fuming sulphuric acid, but only by the rectified acid.

On developing the fuming substance of the Nordhausen sulphuric acid by warmth, and introducing it into iodine-starch, it decolorizes it immediately. Even by warming in a phial one part of black oxide of manganese and two of Nordhausen sulphuric acid, with the view of converting the sulphurous acid that might perhaps be contained in the oil of vitriol into sulphuric acid, and introducing from time to time the vapours into iodine-starch, the latter was deprived of colour even after the mixture had been boiling for a quarter of an hour. Still it would be a mistake to ascribe the quality of decolorizing the iodine-starch to the fuming substance, which, according to F. C. Vogel's and Dœbereiner's and Bussy's experiments, is nothing but sulphuric acid free from water; misled by the peculiar, and as yet imperfectly explained, affinity of the fuming substance to another colouring matter—indigo. On the contrary, the iodine-starch was found in these experiments the most powerful re-agent on the sulphurous acid. For by a repeated

repeated treatment of Nordhausen sulphuric acid with black oxide of manganese, I obtained at last the fuming substance in such a state that, being perfectly free from sulphurous acid, it no longer decolorized the iodine-starch. Another experiment gave a further proof that it is only the sulphurous acid contained in the Nordhausen sulphuric acid which discolours the iodine-starch. For by introducing only as many drops of this fuming sulphuric acid into the starch as will just decolorize it, a little shaking of the liquid in the glass will reproduce the lost colour sometimes in a surprisingly short time. The restoration also takes place by exposing the iodine-starch, decolorized as stated, for some time to the influence of the atmosphere. But the colour is reproduced still more quickly by adding a few drops of nitric acid, just as it is in iodine-starch decolorized by sulphurous acid.

If the colour is removed by sulphuretted hydrogen, it is only restored by nitric or sulphuric acid, if no more sulphuretted hydrogen has been employed than was just necessary to remove the colour. For if the sulphuretted hydrogen be in excess, the colour can only be restored by the liquid's being boiled, and thus the excess of sulphuretted hydrogen removed. I made the attempt to remove this excess by freezing, but did not succeed: whence it may perhaps be asked, whether the separation of the hydrosulphuret of arsenic by freezing depends only on the expulsion of the excess of sulphuretted hydrogen, or on the power of crystallization, favoured by the cold.

It is also deserving of notice, that a decoction of onions decolorizes iodine-starch, and that the colour is afterwards restored by sulphuric acid, just as in the case of arsenic.—Of this, however more hereafter.

In conclusion, I would make the following remark on iodine-starch in its relations to arsenic. Neither Brugnatelli nor any one else has noticed that it is only fresh iodine-starch which is applicable as a re-agent on arsenic. Yet in the lectures in our university it was found that, unless perfectly fresh, it was useless for such experiments:—even if it was but one day old it was of no use, as will be seen in the sequel.

Iodine-starch (made by dropping a solution of iodine in alcohol into a solution of boiled starch in cold water, which had become clear by standing) was distributed in equal quantities and strength of colouring into four glasses. The glass *a* contained iodine-starch just prepared; *b*, such as was one day old; *c*, such as had been a fortnight in a closed glass; and *d*,

such as had been for a fortnight in an open glass. The starch in glass *a* was decolorized by four drops of a solution of arsenic, and that in glass *b* by twelve drops; whilst twenty-four drops of the same solution did not perfectly decolorize the iodine-starch in glasses *c* and *d*. After two hours the liquid in glass *c* was completely colourless, but that in *d* was still violet. The restoration of the colour was perfectly effected in glass *a* by eight drops of sulphuric acid, and in *b* less perfectly by twelve drops; the liquid in *c* and *d* received its blueish colour again after the addition of sixteen drops, but did not assume a full blue even with a greater addition of sulphuric acid. It would perhaps not be easy to determine what change may have taken place in the iodine-starch by a short exposure to the atmosphere, in which the colour suffers no alteration whatever.—Perhaps I shall find an opportunity to make further experiments on the subject.

3. *Affinity of the Decoction of Onions for some Re-agents.*

As it has been frequently said that the decoction of onions bears the same affinity for the re-agents as arsenious acid (See Berzelius *Lehrbuche der Chemie*, vol. ii. p. 154), the subject was considered in our lectures, in order to show the similarity of the precipitates as they appear at first, and also the difference which is perceptible on a closer inspection.

Repeating this examination, I made the following experiments. A small onion was cut and boiled for a quarter of an hour in distilled water, and the decoction being strained off, it was treated with the following re-agents:

1. The red solution of camelion mineral was changed yellow.

2. The blue solution of ammoniated copper turned to a greenish hue.

3. Iodine-starch was decolorized, and the blue colour restored by the addition of sulphuric acid.

4. Sulphuretted hydrogen produced no change in the decoction.

5. Lime-water coloured it yellow, and produced after a little standing a yellowish deposit, but which was not dissolved, like the arsenite of lime, in salts of ammonia, nor was it altered by weak acids.

6. Nitrate of silver produced a white precipitate, which increased and became yellow by the addition of ammonia. On more ammonia being added it disappeared, but was reproduced on being neutralized with nitric acid; and again disappeared

peared with an excess of this acid,—quite similar in this respect to the arsenite of silver. If, however, this experiment be repeated several times, the yellow deposit does not re-appear; and instead of it a brownish one is produced after the lapse of several hours, which might be compared to that arising from nitrate of silver and arsenic acid.

Thus the three first-named re-agents have the same relations with a decoction of onions as with a solution of arsenious acid; but sulphuretted hydrogen, lime-water, and nitrate of silver exhibit distinct differences.

XXXVI. *On the Construction of Sundials; with a Table for that purpose.* By Mr. M. Smith.

To the Editor of the Philosophical Magazine and Journal.

Sir,

THE following table for the construction of sundials is carefully computed in conformity with the principles demonstrated in my letter published in your last Number, page 168. All similar tables which I have hitherto seen are extremely erroneous, being calculated on the supposition that the shadow is cast from the centre of the sun; and therefore any dial constructed from such tables must invariably err one minute of time from the truth. The following table is computed on the principle that the shadow is cast from a point in the sun's disc one minute of a degree within that limb which is nearest to the meridian, and consequently fifteen minutes from the sun's centre. The shadow therefore coincides with the meridian at one minute of time from noon. This remark must be particularly attended to, as it follows that the hour-angle for noon is negative; consequently the upper line in the table, or that standing opposite to $0^h\ 0^m$ is to be set off from the meridian in the contrary direction from all the others, which will have the effect of contracting the double hour-line for twelve o'clock, so as to render it somewhat less in thickness than the gnomon or style of the dial.

I remain, sir,

Your obedient servant,

October 5, 1825. M. Smith.

The Angles which the Hour-lines make with the Meridian in a horizontal Dial, for each Degree of Latitude in England.

Hour from Noon.		Latitude.													
		50°		51°		52°		53°		54°		55°		56°	
H.	M.	°	′	°	′	°	′	°	′	°	′	°	′	°	′
0	0	0	12	0	12	0	12	0	12	0	12	0	12	0	12
	15	2	41	2	43	2	46	2	48	2	50	2	52	2	54
	30	5	34	5	39	5	43	5	48	5	52	5	57	6	1
	45	8	29	8	36	8	42	8	49	8	56	9	3	9	9
1	0	11	24	11	34	11	43	11	52	12	1	12	10	12	19
	15	14	22	14	34	14	46	14	58	15	9	15	20	15	30
	30	17	24	17	38	17	52	18	6	18	19	18	32	18	44
	45	20	30	20	46	21	2	21	17	21	32	21	47	22	1
2	0	23	39	23	57	24	15	24	32	24	49	25	5	25	21
	15	26	53	27	13	27	33	27	52	28	11	28	28	28	45
	30	30	13	30	35	30	56	31	16	31	36	31	55	32	14
	45	33	40	34	3	34	25	34	46	35	7	35	27	35	47
3	0	37	13	37	37	38	0	38	22	38	44	39	5	39	25
	15	40	53	41	18	41	42	42	4	42	26	42	47	43	8
	30	44	42	45	6	45	30	45	53	46	15	46	36	46	57
	45	48	38	49	3	49	27	49	49	50	10	50	31	50	52
4	0	52	43	53	7	53	30	53	52	54	13	54	33	54	53
	15	56	56	57	19	57	41	58	1	58	21	58	40	58	59
	30	61	18	61	39	61	59	62	18	62	36	62	53	63	10
	45	65	48	66	6	66	24	66	41	66	57	67	12	67	27
5	0	70	25	70	40	70	55	71	9	71	23	71	35	71	48
	15	75	8	75	20	75	32	75	43	75	53	76	3	76	13
	30	79	56	80	4	80	12	80	20	80	27	80	34	80	41
	45	84	47	84	52	84	56	85	0	85	4	85	8	85	11
6	0	89	41	89	41	89	41	89	41	89	41	89	42	89	42
	15	94	34	94	30	94	26	94	23	94	19	94	16	94	13
	30	99	26	99	18	99	10	99	3	98	56	98	49	98	43
	45	104	14	104	2	103	51	103	41	103	31	103	21	103	12
7	0	108	58	108	43	108	28	108	15	108	2	107	49	107	37
	15	113	36	113	18	113	1	112	44	112	29	112	14	111	59
	30	118	6	117	46	117	26	117	8	116	50	116	33	116	16
	45	122	29	122	7	121	46	121	25	121	5	120	46	120	28
8	0	126	44	126	20	125	57	125	35	125	14	124	54	124	35
	15	130	50	130	25	130	1	129	39	129	17	128	56	128	36
	30					133	58	133	36	133	14	132	53	132	32
	45											136	42	136	21

XXXVII. *Errors in* Piazzi's *Catalogue of Stars.*

THE high reputation which M. Piazzi's catalogue of stars so justly maintains, and the constant use which is made of it in every active observatory, render it necessary that the detection of any errors should be as widely circulated as possible. We trust, therefore, that we need not apologize to our readers for the insertion of the following list. A considerable portion of the errors, therein stated, were communicated to a gentleman in this country (highly distinguished for his mathematical and scientific attainments) by M. Piazzi himself; and some others by M. Cacciatori, his worthy successor at the observatory at Palermo: the remainder (which are distinguished by an asterisk annexed) have been communicated by one of our correspondents, who has had frequent occasion to refer to this valuable work.

Hora et Numerus.	Columna.	Errata.	Corrige.	
0. 28	Motus in Æ et D	M	B	*
28	Præc. in Dec. .	20,00	20,06	
37	Declin.	. . . . .	42. 40. 45,0	
40	Declin.	. . . . .	44. 20. 49,7	
64	Motus in Dec. .	F. B. P.	F. B. M. P.	
76	Motus in Dec. .	−0,06	−0,05	
134	Motus in Dec. .	B. F.	F. B. P.	
232	Motus in Æ . .	+0,2 F. P.	+1,2 F. P.	
234	Declin.	. . . . .	86. 4. 14,5	
I. 10	Declin.	13. 18. 59,9	3. 18. 59,9	
31	Num. obs. in Dec.	9	4	
102	Declin.	73. 16. 13,8	73. 16. 13,3	?
126	Nomen	120 π	102 π	*
140	Nomen	γ	ν ?	*
150	Nomen	106 γ	106 ν	*
251	Nomen	γ Fornacis	ν Fornacis	
in notis	ad 232	Præc. ad aust.	Præc. 3′ ad aust.	
II. 48	Num. obs. in Dec.	4	8	
187	Æ	. . . . .	39. 44. 18,0	
192	Nomen	. . . . .	43 σ Arietis	
254	Declin.	. . . . .	40. 10. 27,0	
IV. 27	Nomen	50 $\alpha 2$	50 ω^2	*
81	Nomen	234 C. A.	334 C. A.	
V. 180	Nomen	116 Tauri	126 Tauri	*
300	Præc. in Dec. .	9,76	0,76	*
321	Stellarum num.	821	321	*
322	Stellarum num.	222	322	*
VI. 87	Num. obs. in Æ	15	10	

Hora et Numerus.	Columna.	Errata.	Corrige.	
VI. 287	Motus in Æ et D	. . . . .	dele	*
288	Motus in Æ .	. . . . .	+0,05 M.	*
288	Motus in Dec. .	. . . . .	−0,02 M.	*
VII. 182	Præc. in Æ . .	55,54	54,54	
VIII. 72	Nomen	754 C. A.	756 C. A.	*
221	Motus in Æ et D	. . . . .	dele	*
222	Motus in Æ . .	. . . . .	−0,03 M. B.	*
222	Motus in Dec. .	. . . . .	−0,08 M. B. C.	*
in notis	ad 135	157	357	*
IX. 136	Declin.	. . . . .	26. 15. 38,0	
174	Nomen	29 ν	29 υ ?	*
181	Æ	. . . . .	39. 15,0	
182	Æ	. . . . .	40. 51,0	
X. 238	Stellarum num.	239	238	
239	Stellarum num.	238	239	
XI. 116	Nomen	91 ι	91 υ	*
XII. 60	Num. obs. in Dec.	18	13	
103	Motus in Æ et D	. . . . .	dele	*
104	Motus in Æ . .	. . . . .	−0,35 M.	*
104	Motus in Dec.	. . . . .	−0,17 M.	*
183	Nomen	532 May.	522 May.	*
pag. 89	caput Declin. .	An. Præc. Motus	Motus An. Præc.	*
XIII. 295	Stellarum num.	395	295	*
XIV. 62	Nomen	596 May.	569 May.	*
XV. 118	Nomen	614 May.	611 May.	*
193	Num. obs. in Dec.	11	15	
221	Motus in Dec. .	. . . . .	+0,05 B. P.	
263	Motus in Dec. .	−0,10 P. P.	−0,10 M. P.	
277	Æ	15^h. 68	15^h. 58	
XVI. 137	Nomen	657 May.	654 May.	*
248	Nomen	606 May.	666 May.	*
280	Declin.	32. 52. 0,0	33. 52. 0,0	
XVII. 44	Declin.	34. 59. 25,0	34. 56. 25,0	
166	Præc. in Dec. .	2,79	2,81	
XVIII. 41	Declin.	26. 29. 38,0	16. 29. 38,0 ?	*
112	Nomen.	736 May.	737 May.	*
234	Præc. in Dec. .	8,99	3,99	*
XIX. 379	Æ in temp. . .	18^h. 54	19^h. 54	
417	Nomen	829 May.	820 May.	*
XX. 253	Declin.	. . . . .	7. 45. 5,8	
489	Stellæ num. . .	486	489	
XXI. 256	Declin.	. . . . .	56. 40. 40,3	
XXII. 46	Motus in Dec. .	−0,13 M.	−0,18 M.	
316	Nomen	982 M.	953 M.	*
XXIII. 12	Præc. in Dec. .	13,44	19,44	*

XXXVIII. *On Tungsten.* *By* F. Wöhler*.

BEFORE describing some combinations of tungsten, which in my opinion have not hitherto been known, I will indicate one of the processes I employed to procure pure tungstic acid. I had noticed that on heating a mixture of tungstate of potash and of hydrochlorate of ammonia, the tungstic acid is reduced into oxide by the hydrogen of the ammonia, which separates on the mass being dissolved in water. This property I have applied in the preparation of tungstic acid in the following manner: The mixture of pulverized wolfram and carbonate of potash being melted together, the tungstate of potash is dissolved in water, and a sufficient quantity of hydrochlorate of ammonia added; the mass is then evaporated to dryness, and ignited and fused in a Hessian crucible till the sal ammoniac be entirely decomposed or evaporated. On dissolving the melted mass in hot water, a black and heavy powder is obtained, which is the oxide of tungsten. This is boiled with a weak solution of pure potash, for the purpose of carrying off a small portion of supertungstate of potash difficult of solution. When it is desired to have tungstic acid, this oxide is to be heated in an open crucible: it takes fire and burns vividly, changing into a yellow powder.

Oxide of Tungsten.

This oxide was discovered by M. Berzelius, who obtained it by heating tungstic acid in hydrogen gas. The acid at first turns perfectly blue, then gradually dark brown. As I employed for this experiment the crystalline tungstic acid, such as is obtained by the decomposition of crystallized tungstate of ammonia, I obtained a crystalline oxide of a lustre almost metallic, which after burnishing preserved this lustre, and had a dark copper-colour. It may also be obtained under the same form, but with a perfect metallic lustre, and of the most beautiful copper-colour, by bringing into contact a mixture of tungstic acid and zinc filings with diluted muriatic acid. The hydrogen at first turns the tungstic acid blue, and afterwards reduces it to brilliant laminæ of a copper-colour. The oxide thus obtained is only preserved under water; for by contact with atmospheric air it instantly turns blue, and soon changes completely into a yellow acid.

On comparing the character of this oxide of tungsten with the black powder obtained by the treatment of tungstate of potash with sal ammoniac,—which, as I have said, is oxide of

* From *Annales de Chimie et de Physique*, tom. xxix. p. 43.

tungsten,

tungsten,—it might rather be presumed that this is the tungsten in its metallic state, especially when I observe, that this black body acquires a white metallic brilliancy, although a very deep one, when burnished. But the degree of increase of weight it acquires in combustion proves that it is oxide of tungsten, and not the metal itself. Heated in the air it burns long before it reddens; and according to many experiments, 100 parts always combine with 8 parts of oxygen (*i. e.* with the same quantity as the brown oxide) while passing into the acid state; whilst 100 parts of metallic tungsten require nearly 25 parts of oxygen in order to be converted into tungstic acid. It seems to me remarkable that the same combination should exhibit itself under such different forms. It is probable that the different state of the aggregation of its molecules is the cause of this phænomenon, as is also observed in native oxide of iron, cinnabar, &c.

In preparing the brown oxide of tungsten by heating the acid in a current of hydrogen gas, another phænomenon is presented, which I cannot well explain. It is a fact that it is difficult to prepare pure tungstic acid when it once contains a fixed alkali. On using such an acid containing a little potash or soda, to prepare the brown oxide by the hydrogen, this oxide is never obtained, but always metallic tungsten,—a method by which this metal may also be easily procured. It is washed with pure potash, in order to dissolve the difficultly soluble tungstate with which it is mixed: it is then a metallic powder, rather white, very heavy, which, heated in the air, ignites, and of which 100 parts increase by nearly 25 parts.

Combination of the Oxide of Tungsten with Soda.

On melting and igniting the neutral tungstate of soda in hydrogen gas, no action of the latter substance on the former is observed; but if the same experiment be made with supertungstate of soda, the surface of the mass soon assumes the colour and metallic brilliancy of copper, which is gradually communicated to the whole mass. On cooling, the colour becomes a gold-yellow; and if the mass be then treated with water, the neutral tungstate of soda is dissolved, and a heavy crystalline powder, of the colour and almost the metallic lustre of gold, is left. The best method of making the experiment is by using a glass tube which has a bulb in the middle, or by making the salt almost red-hot in a current of dry hydrogen gas. The mass is then boiled with water. The remaining powder is first digested in concentrated muriatic acid, in order to decompose the insoluble tungstate attached to it: it is then boiled with a solution of pure potash; and at last washed in water.

water. It is necessary to employ all these precautions if the combination is to be obtained in its pure state.

The super-tungstate of soda may be prepared by adding tungstic acid to the neutral salt while melting, until the last portions will no longer dissolve.

The yellow metallic substance, which I shall show to be a combination of oxide of tungsten with soda, is crystallized in regular cubes, which are larger in proportion to the slowness of the operation by which they have been formed. Sometimes cavities are found in the reduced saline matter, the sides of which are formed by an aggregation of small and very brilliant cubes. This combination has, as I said before, a perfect metallic brilliancy even when rubbed on paper; its colour scarcely differs from that of gold, and on looking at such a crystalline powder in the sun, it displays a beauty and a brilliancy possessed by few chemical preparations. Suspended as a fine powder in water, and looked at towards the sun, it is, like gold, transparent and of a green colour.

It is decomposed by no acid whatever, not even by boiling aqua-regia. Nothing but concentrated fluoric acid decomposes and dissolves it. The solutions of pure alkalies have no effect on it. Heated in contact with atmospheric air, it changes colour, softens, melts in a uniform manner; and around the substance submitted to the experiment is formed a white enamel which is soluble in water. By then adding an acid, tungstic acid is precipitated. But the whole mass of the combination is never converted into that enamel,—there always remains a part in the middle which does not decompose.

This decomposition is not more complete even in oxygen gas, although it be accompanied by combustion. Here, too, the fusible mass which is produced, forms a stratum which hinders the action of the gas on the interior portions. In a vacuum the combination may be heated without decomposition. It appears therefore very probable that the fusible substance formed in this case is nothing else but tungstate of soda, and that this new body contains at least tungsten and sodium; but it was difficult to decide if those bodies were in the oxidated or in the metallic state. The property of not being decomposed by aqua-regia was opposed to its being regarded as an alloy of tungsten and sodium, and its metallic properties rendered it difficult to admit that this combination was oxidated.

As this combination is not decomposed by the most powerful re-agents, I was obliged to seek for other means; and found that it was decomposed by chlorine,—but only on its being strongly heated in this gas; otherwise there would be no reason why it should not also be decomposed in aqua-regia.

Heated in chlorine, a slight incandescence takes place; chloride of tungsten which volatilizes, and of which I shall speak hereafter, is formed. The other product of the combustion is a greenish mass, which, treated with water, gives chloride of sodium crystallized in cubes. The insoluble green powder is a mixture of tungstic acid with a little oxide; but the quantity of the acid is much larger than that of the chloride and oxide together. The result therefore seems to be, that the combination contains oxygen, the whole quantity of which, before the action of the chlorine, was distributed so as to form soda and oxide of tungsten, and which, by the combination of the chlorine with the sodium, entirely united with the tungsten in order to form tungstic acid. I obtained exactly the same result in employing the chlorine in a perfectly dry state; and by avoiding the least mixture of atmospheric air with the chlorine, in such manner that the formation of the tungstic acid could proceed neither from the oxygen of the water nor from that of the atmospheric air, 0·873 grain of the combination, decomposed by chlorine, gave 0·157 grain of chloride of sodium = 0·089 grain of soda; consequently, 10·6 parts of soda in 100 of the combination. But as I expected to have observed that the half-fused state of the chloride of sodium prevented the complete action of the chlorine on the combination, I tried another manner of analysing this body; viz. by sulphur, —which entirely decomposes it. 0·487 grain of the combination were melted with pure sulphur in a closed crucible of porcelain. The resulting mass had the appearance of sulphuret of tungsten, and weighed 0·55 grain. No sulphuret of sodium could be discovered, either with water or muriatic acid. It was therefore treated with aqua-regia, which converted it into pure tungstic acid. The fluid was made to evaporate on the acid, and then the whole mass ignited. The tungstic acid, placed in a filter, was washed till all the sulphate of soda which had been formed was dissolved. The pure acid, dried and heated, weighed 0·45 grain. This quantity corresponds with 86·2 parts of oxide of tungsten in 100 parts of the combination: the remainder, therefore, is the quantity of soda = 13·8 parts. It seems therefore that this combination is composed of,

	Atoms.	By calculation.	By experiment.
Oxide of tungsten	4	87·81	86·2
Soda	1	12·19	13·8
		100·00	100·0

It will be seen that the oxygen of the soda is not half the quantity which is required to convert the oxide of tungsten into acid: for this purpose a part of the oxygen of the oxide of

of tungsten must be employed; and consequently a corresponding part of tungsten set free, and combined (as I have said before) with chlorine. From this reason it is evident that this new combination can only contain the tungsten in the state of oxide.

I have tried without success to produce this body by the direct combination of the oxide of tungsten with soda. By heating those bodies together I obtained metallic tungsten and tungstate of soda.

Lastly, I also tried to produce an analogous combination with potash, and treated the super-tungstate of potash in the same manner as the corresponding salt of soda. I obtained a metallic mass of a whitish colour, being pure metallic tungsten.

Chloride of Tungsten.

Sir H. Davy was the first who found that tungsten heated in chlorine burns and produces a white substance, which is decomposed by the action of water into tungstic acid and hydrochloric acid: but nothing further seems to be known on the subject. I have found that there are three different combinations of tungsten and chlorine.

Perchloride of Tungsten.

This is always produced, and nearly in a pure state, on heating the black oxide of tungsten in chlorine;—with the brown oxide, tungstic acid is formed at the same time. The combination is effected with an evolution of light; the glass globe in which the operation is performed is filled with a thick yellow smoke, which is condensed into scales of a yellowish-black colour, and which at last forms a thick sublimate that exteriorly perfectly resembles native boracic acid. In contact with atmospheric air this chloride changes, according to the hygroscopic state of the air, within some hours or some days, into tungstic acid; and at the same time hydrochloric acid is disengaged. With water this decomposition is more rapid, although not instantaneous; very pure tungstic acid is deposited, and weak hydrochloric acid is formed at the same time. In ammonia it dissolves with a slight noise and an evolution of heat. It volatilizes in a low temperature without first melting, and its vapour is of a dark yellow colour.

Heated on platinum foil by a spirit-lamp, it is decomposed at the moment when it is volatilized, by the action of the aqueous vapour formed by the combustion of the alcohol: vapour of hydrochloric acid is formed; and the tungstic acid which is produced, forms above the flame a luminous smoke, and then disperses in large and very light flakes.

The chloride of tungsten being decomposed in water into tungstic acid and hydrochloric acid, must correspond in its composition with the tungstic acid;—viz. it must be composed of 3 atoms of chlorine, and 1 of tungsten, thus:

	Atoms.	
Chlorine	3 . . .	35·9
Tungsten . . .	1 . . .	64·1
		100·0

By an approximative experiment, 0·166 grain of this chloride, dissolved in ammonia, evaporated and heated, yielded 0·13 grain of tungstic acid = 62·65 parts of tungsten for 100 parts of the chloride.

Protochloride of Tungsten.

This compound is always formed, with a very slight mixture of the others, on heating metallic tungsten in chlorine. The metal ignites and is completely converted into chloride, which is presented sometimes under the form of an aggregation of fine tender needles of a deep red, resembling wool, but more frequently as a compact melted mass of a deep red colour, with a shining fracture nearly like that of cinnabar. It easily melts, and boils before it is volatilized. Its vapour has a red colour, deeper than that of nitrous acid. In water this chloride soon becomes violet, decomposing gradually and completely into oxide of a violet colour, and hydrochloric acid. This chloride is dissolved, with evolution of hydrogen gas, in a solution of pure potash; it produces tungstate of potash, and chloride of potassium. With caustic ammonia, hydrogen gas is also disengaged; but in this instance a yellowish solution is formed, which loses its colour: on being heated very slowly, brown oxide of tungsten is deposited.

This chloride seems to bear an analogy with the oxide, and must be composed of,

	Atoms.	
Chlorine	2 . . .	26·79
Tungsten . . .	1 . . .	73·21
		100.00

The third combination of chlorine with tungsten,—on the composition of which, however, I have made no experiment, and on which I shall make no conjecture,—is generally formed with the perchloride, although in very small quantity. I obtained it once in a larger quantity on heating sulphuret of tungsten in chlorine. This third chloride is the most beautiful of any: it is formed in transparent needles of a beautiful red colour, and often of great length; it melts very easily by a gentle heat, and crystallizes on cooling in long transparent radii

radii which are spread over the glass. It is more volatile than the other chlorides; its vapour has the colour of nitrous acid. In contact with the atmosphere it changes instantaneously into tungstic acid. Thrown into water it effervesces like quicklime, and heat is evolved; a particular noise is heard as in the slaking of lime, and in a moment it is entirely changed into tungstic acid.

XXXIX. *On the Existence of Iodine in Minerals.* By M. Vauquelin*.

IODINE has as yet been found only in a few vegetables and some sea mollusca. M. Cantu, professor of chemistry at Turin, however, has lately discovered it in the mineral water of Asti; but no one to my knowledge has as yet found it in combination in minerals.

M. Jos. Tabary having sent me a few weeks ago some argentiferous minerals,—which he had partly bought of some of the native tribes of South America, and partly collected himself in the vicinity of Mexico, within a circle of 25 leagues from that city,—in order that I might ascertain for him the quantity of silver and gold (if the latter should be contained in it), I was so fortunate as to make the discovery which I shall have the honour to communicate to the Academy.

One of these minerals, called *virgin silver of serpentine*, and the physical properties of which are, 1st, a whitish colour on its surface worn by friction, presenting grains of metallic silver; 2nd, a lamellar fracture of a yellowish-green colour, with some parts black and of metallic silver,—is that in which I found the iodine.

Twenty grammes of this mineral treated with nitric acid effervesced, and evolved some nitrous gas towards the end of the operation. After having boiled it for some time, the liquid diluted with water presented two substances: the one very heavy, becoming quickly precipitated; the other, light, remaining a long time suspended in the fluid. They were separated from each other by decantation, washed, and dried.

The first, which weighed $6\frac{42}{100}$ grammes, was easily melted before the blowpipe, producing a purplish flame; and after some time a globule of silver appeared in the midst of a melted substance, which spread over the charcoal like chloride of lead. The edges of the charcoal were invested with a yellow powder.

The other substance, which was brown, weighed $2\frac{70}{100}$

* From *Annales de Chimie*, tom. xxix. p. 99.

grammes.

grammes. It was inflamed by heat, giving out a smell of sulphurous acid, and leaving as a residue sulphuret of lead mixed with a little iron, which weighed 1·58 gramme.

The first substance, weighing $6\frac{42}{100}$ grammes, treated with muriatic acid aided by heat, gave the latter a reddish-brown colour, and produced a slight effervescence, with the smell of chlorine. As the heat increased, this effervescence also increased, and a fine violet-colour was soon developed; when, in order to preserve this violet matter, the vessel was immediately withdrawn from the fire.

At the bottom of the acid remained a yellow substance containing gray particles, which were dissolved in the hot water with which the substance was washed.

This water had acquired a reddish-brown tinge, and the property of imparting to a solution of starch a fine blue colour.

Suspecting, although the circumstance appeared so extraordinary, that this violet vapour was produced by iodine, the above muriatic solution, after having been a little diluted with water, was submitted to distillation. It was with pleasure we saw our supposition realized: for soon the violet vapours, on rising, crystallized against the sides of the adopter and balloon adapted to the retort, assuming the acicular form and the colour peculiar to iodine; but the acid was not entirely decolorized. Although the yellow substance had been boiling for some time with the muriatic acid, it was not entirely decomposed; for having melted 2·38 grammes of it with potash, and washed the produce with water, we obtained an alkaline solution, which, saturated with sulphuric acid and mixed with starch-water, produced, with the addition of some drops of solution of chlorine, a very fine blue colour. The substance which did not dissolve in the water was metallic silver in the state of powder, weighing 1·63 gramme.

Assured therefore, by the preceding experiments, of the presence of iodine in the argentiferous mineral, we endeavoured to obtain it by a more direct process, which would permit us to determine its quality and to ascertain the mode of combination in which it occurs.

We accordingly heated 5 grammes of it in powder with 2 grammes of caustic potash and a little water to facilitate their mixture. The substance having been heated for some time, we washed it out in water; and after having decanted the latter, washed the residuum till all the alkaline matter was removed. This residuum was of a dirty-yellow, and weighed 4·46 grammes.—We shall return to its examination presently.

A portion of the alkaline solution saturated with nitric acid took

took a yellow tinge, and had the property of giving a blue colour with starch on the addition of a few drops of chlorine: it was precipitated blackish-brown by nitrous acid, and red by nitrate of mercury.

The 4·46 grammes left after the action of the potash, treated with diluted nitric acid, dissolved with effervescence; but a yellowish substance remained, resembling, except in colour, chloride of silver. Washed and dried, it weighed 80 centigrammes; heated, it turned orange, and became of a greenish-yellow again after cooling.

We have ascertained this substance to be iodide of silver, which is proved by the alkali's not having decomposed it, although in excess. The quantity of silver dissolved by the nitric acid was $41\frac{1}{2}$ centigrammes.

Thus the potash having carried off 50 centigrammes from the 5 grammes of ore (which can be nothing but iodine); and as we obtained, besides, 80 centigrammes of iodide of silver, which contain according to modern chemists $42\frac{1}{2}$ of iodine,—it follows that these grammes of ore contain 92·50 of iodine, which, divided by 5, give 18·50 per cent.

No doubt therefore could remain as to the existence of iodine in the ore in question: however, we were desirous of knowing whether we could obtain the hydriodate of potash in a crystallized state. For this purpose we saturated with sulphuric acid the excess of alkali contained in the solution; and after having evaporated it to dryness, we treated it with alcohol of the sp. gr. ·867, for the purpose of isolating the sulphate of potash. We then expelled the alcohol; and the mother water left to spontaneous evaporation, produced square prisms which had all the properties of ordinary hydriodate of potash.

Henceforward iodine may be considered as one of the elements of minerals: and this fact will be a motive for chemists not to neglect to search for it when analysing metalliferous minerals, especially such as contain silver; for iodine, like chlorine, exerts a great influence on this metal.

The question now is, with which of the substances contained in the silver-ore it is combined. It will be recollected that we found in it sulphur, silver, lead, and carbonate of lime, which serves as its gangue. The carbonate of lime may be excluded at once; and the difficulty lies between the sulphur, silver, and lead. It is not very probable that this substance should be combined with the sulphur; this as well as the lead being separated by the action of the nitric acid on the ore, even when diluted: it is more probable that the iodine is combined with the lead and part of the silver. On the other hand, if we consider that in proportion as the nitric acid dissolves the metallic

tallic silver and decomposes a part of the sulphuret of lead, iodide of silver is precipitated on which the nitric acid has no action, we are led to believe that the iodine is combined with the silver. Moreover, this opinion appears fully confirmed by the circumstance that a certain quantity of iodine may be extracted from the silver ore by boiling it for some time with ammonia: besides, it is known that iodine, like chlorine, has a great affinity for silver.

I shall deposit the remainder of this specimen in the Cabinet at the Jardin du Roi, to serve for comparison with others in case its locality should be discovered at some future time.

XL. *A Method of Measuring an inaccessible Line: with Observations on Mr.* Newton's *Cross for Surveying.*

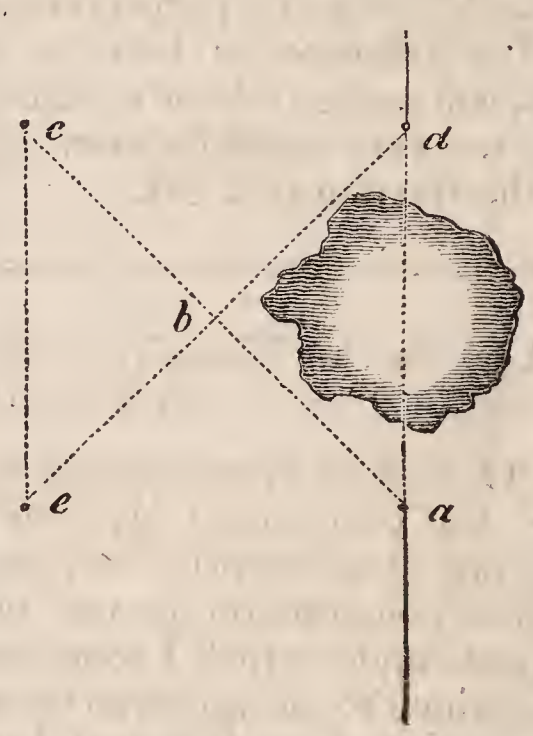
To the Editor of the Philosophical Magazine and Journal.

Sir,

HAVING many years since occasion to measure a long line which fell across a pond of considerable breadth, the following method of obtaining the distance occurred to me at the time; and as it has, I believe, never been published, nor have I ever seen the method used by any other person, if you think it deserving of notice, its insertion in your Journal will oblige, sir, Yours, &c.

Oct. 4th, 1825. Leander.

Measure a straight line from *a* to *c* and from *d* to *e*, making *b c* = *b a* and *b e* = *b d*; then by similar triangles *e c* is = to *a d*; whence by measuring *c e* we get the distance *a d* as was required.



In Number 326 of the Phil. Mag. is given a description of an improved cross for the use of land-surveyors by Mr. Isaac Newton.

Now I cannot exactly agree with this gentleman as to the advantage to be derived from the use of this new instrument. Suppose, for instance, a diagonal line to run due north and south, and the staff A B D G (see vol. 65, page 431) to be fixed in it, the cross C revolving on the arm F H can only give the perpendicular correct when due north and south of the staff, or when it coincides with the diagonal line, as in revolving round

round the staff, it is out of the diagonal line in any other position, and of course throws both the diagonal and perpendicular out of their true directions.

In measuring diagonals, &c. by means of the chain and *common* cross, Mr. Newton says, "The young practitioner frequently is obliged to prick his staff in six or seven different places before he can succeed in finding the diagonal, &c., but in using the above he will seldom find it requisite to ground his staff more than once, in order to answer the same purpose." Now it is evident this new instrument can be placed the first time only 18 inches, at most, nearer to the required point than the *common* cross (18 inches being the length of the revolving arm F H). This distance on the diagonal on either side of the point where the true perpendicular falls, will cause a difference in the lengths of the perpendiculars, supposing them to be one, two, three, four, and five chains in length respectively, of $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{15}$, $\frac{1}{20}$, and $\frac{1}{25}$ part of an inch only, in their respective lengths; and if a cross can be placed within the distance of 18 inches, on the first trial, of the true point where the perpendicular falls on the diagonal (as Mr. Newton says it is seldom requisite to ground his staff more than once), I really cannot see the necessity for pricking the staff in five or six different places afterwards. Errors, I may say, of almost infinitely greater magnitude arise from inattention to the correct length of the chain, and to the chain-leader not putting down his pins in a perpendicular direction, &c.

The difference of 1-5th of an inch, or even of an inch or two, will make little or no difference in the result, as I believe it is not very usual for even *professional* surveyors to read off to the fraction of a link.

XLI. *Further Thoughts on Mr.* Herapath's *Demonstration.* *By* T. S. Davies, *Esq.*

TO find an interpretation different from that upon which I had commented given to Mr. Herapath's demonstration did not at all surprise me, as I had for some time hesitated which construction to put upon it myself; and of the two aspects under which I conceived it may be considered, I chose that which to me appeared the most feasible, as that under which the author himself would have it viewed. By considering $1 + 2_r + 3_r + \ldots$ and $1 + 2_v + 3_v + \ldots$ as representations of the series just deduced for integer values, rather than as an arbitrary denotation of the development for fractional ones (when

no such values of v and r had even been in the most distant manner suggested), was not only the most *natural* course, but seemed to be far the least objectionable mode of proceeding. It is true the title of the article led us at once to see the ultimate object of the inquiry; but it by no means intimated that in the stage at which we had arrived, a different class of values from that we had been previously considering was to be given to the indices. It is not usual to employ quantities in an elementary investigation with a new import, without expressly stating that the import of them is changed at that step, nor can a due regard to perspicuity be consistent with such an omission. Every one conversant with subjects of this nature must be aware how vaguely and imperfectly the mere title of a paragraph must necessarily define the character of the symbols which are introduced into an investigation. It will be recollected that not the slightest intimation had been given that it was concerning the symbols r and v that the ultimate results were sought; and in reality, the passage quoted by P. Q. in support of those symbols having been *introduced* as fractions, appears quite as much like an extension of the signification which they had previously possessed,—if indeed that be the meaning which Mr. Herapath himself wished to convey by it.

However, it is a matter of little importance what construction the passage will bear, or even which is its most obvious interpretation; it is as little, perhaps, in reference to the principle for which I contend,—whether Mr. Herapath has failed in his usual precision of expression, or whether through haste or misconception I have mis-stated his processes. It is sufficient, in the present case, if when the most favourable statement of both views is made, that my principle equally applies, and that the example which I have selected of its application is equally pertinent, whichever interpretation of the selected process we may fix upon. If I can show that under the interpretation of P. Q. the demonstration is even more faulty than that I had already considered, then I think a step will be gained in the application of a great and pervading principle,—of a principle in itself so obvious, that I am astonished it was not adopted when *experiment* was made the basis of mental disquisition, even though it had escaped notice when Natural Philosophy was first placed upon the same foundation: and still more surprised am I to find that it is not unhesitatingly admitted amongst our very axioms in this talented and inquiring age. It is not a little remarkable that the only relics of the metaphysics of Pythagoras and Plato are to be found inwoven with the principles of that science which claims for itself

itself the utmost degree of exactitude and certainty. Let us then divest the science of its artificial connexion with the mysticism of innate ideas and universal truths cognisable by us through other media than sensation and experience! When we can fairly understand the evidence upon which our knowledge actually rests, we shall have done much towards weeding its details of many deformities and irrelevant operations, and rendering not only the means of acquisition but those of discovery more facile than the most sanguine of us can at present form any notion of!

Let us consider, then,—if indeed the error be not too obvious to need remark at all,—the position laid down by Mr. Herapath: when

$$r + v = n = \text{indeterminate } \textit{integer},$$

that r and v "*will in point of value be independent.*" I take for granted here, that the distinctions in "value" here refer to integer or fractional values of the symbols; it is the *only* interpretation of which I can perceive the application in the present inquiry. Surely then r and v are *mutually dependent*: if not, the equation $r + v = \textit{integer}$ is destroyed. Mr. Herapath's conditional equation and his conclusion cannot therefore *simultaneously* exist; and of course the reasoning which is built upon that simultaneous existence must also be subverted by this consideration. Indeed the "*independence* of the functions" r and v is just of the same character as would be the assumption of independence of an angle and its complement, or of a number and its reciprocal: and a demonstration built upon assumptions like these would be equally as valid as Mr. Herapath's demonstration of the binomial.

Mr. Herapath proceeds—"And because in the two right hand members of this" (an equation derived *solely* from his demonstration for integer values and the principles of combinations, in conjunction with $n = r + v$), "r and v are *independent variables*, these members when duly reduced *must not contain any product of the powers of the variables; for if they did, the function of either variable would be affected by the changes of the other variable*, WHICH IT SHOULD NOT." Will not then a variation take place in any specific function of a fraction and its complement, by changing the values of one of these? It is incumbent, at all events, on Mr. Herapath to show the truth of such a principle, before we can admit its application here—a task which when accomplished will introduce a new species of "mathematical magic," more refined, and not less ludicrous, than that upon which Berkeley and Maseres exercised their castigating irony.

It is for the reason above given that we are to admit the identity of these two equations: (Phil. Mag. vol. lxv. p. 324.)

$$\frac{(r+v)^{q-1}+a(r+v)^{q-2}+a_2(r+v)^{q-3}+\dots}{1\cdot 2\dots(q-1)} = q_r + 2_v(q-1)_r + \dots q_v,$$

and

$$\frac{\left(r^{q-1}+v^{q-1}\right)+a\left(r^{q-2}+v^{q-2}\right)+\dots}{1\cdot 2\dots(q-1)} = q_r + q_v.$$

And further, for the *independence of* v *and* r, we are to admit, that "this equation *evidently* gives

$$q_r = r\cdot\frac{(r-1)(r-2)(r-3)\dots}{1\cdot 2\dots(q-1)} \text{ and } q_v = v\frac{(v-1)(v-2)\dots}{1\cdot 2\dots(q-1)}$$

which completes the proof."—(Ibid.)

As this latter conclusion is derived by taking v and r separately equal to zero, a momentary recurrence to the *original equation of condition* ($r + v = integer$) would have convinced Mr. Herapath that if every preceding step had been perfectly legitimate, still these resulting equations are *not for fractional values of* r *and* v, *but for integer only:* and I am confident that that gentleman will see the force of this suggestion, and withdraw his claims to having given the "most complete and general demonstration which has yet been published of this celebrated theorem." His demonstration I confess is *as good*, but *not better*, than any other that has been given: and more, it is as good as any that ever *will* be, or that ever *can* be given.

It is now sufficiently clear, that in either view of Mr. Herapath's demonstration the same fallacy is involved,—the same gratuitous assumption employed; viz. an extension of the values of r and v to forms not consistent with the conditions originally stated as the basis of the investigation.

I hope I shall not be charged, even by implication, with any wish to do injustice to the mathematical labours of Mr. Herapath. No man can entertain for that gentleman's indefatigable spirit and splendid powers of intellect a more sincere respect than I have ever done. The mistake of Mr. Herapath is a very general one,—it originates in a principle which has obtained universal credit,—and is so far from involving anything derogatory from his mathematical character, that it is almost invariably found amongst the writings of men of the very first order of scientific merit.—This remark would have been unnecessary, had not P. Q. insinuated that my wish was "to overwhelm in the ruins of the binomial demonstration" the

the labours and discoveries of Mr. Herapath "in other parts of his writings." Nothing could be further from the truth than such a charge. The majority of Mr. H.'s writings have not been upon subjects in which the demonstration of the binomial, or even the truth of the binomial, is at all concerned. I have objected to a very small number of his *methods of demonstration*, but have not in a solitary instance objected to a single mathematical fact to which he lays claim. As well might P. Q. charge me with denying the truth of the binomial theorem, as with involving in the ruins of a single demonstration most of the labours of Mr. Herapath's life!

Bristol, Oct. 12, 1825.

Erratum.—In line 14 of my former paper (p. 115), *for* "that perfect one of all," *read* "that most perfect one of all."

XLII. *Correction in Dr.* Ure's *Paper on the latent Heat of Vapours. By* T. Tredgold, *Esq.*

To Richard Taylor, Esq. &c.

Sir,

In Dr. Ure's paper on the latent heat of vapours in the Philosophical Transactions for 1818 (see Phil. Mag. vol. liii. p. 193) a considerable error in the mode of calculating the results of the experiments has not been noticed. When it is corrected, we have for the latent heat of steam, 888° instead of 967°, for that of alcohol 355·1° instead of 442°, &c. The error occurs in the second operation. "From 42·5° to 212° there are 169·5°; one half of which = 84·75°, or in round numbers 84°, is the rise of temperature which would be produced by adding to water at 42·5° its own weight of boiling water; and $\frac{84}{161\cdot7} = 0\cdot52$ is the elevation which 200 grs. would occasion on 32340 grains." But the 200 grains has been added to 200 grs. to reduce its temperature to 84° of excess: hence, either this reduction should not have been made, or 84 should be divided by $\frac{32340}{400} = 80\cdot85$ instead of 161·7.

Calculating by the formula for mixed fluids (Playfair's Nat. Phil. vol. i. art. 317) we have 888·05 = the latent heat of steam, when we suppose, as Dr. Ure has done, that there is no loss of heat in the operation, and that the specific heat of water is the same in the whole range of temperature from 42·5 to 212.

Perhaps the notice of these errors in your Journal will lead to an investigation of this interesting subject.

I am, sir, your most obedient servant,

16, Grove Place, Lisson Grove, Oct. 18, 1825. Thomas Tredgold.

XLIII. *Me-*

XLIII. *Memoir on the Urao (Carbonate of Soda).* By Messrs. Mariano de Rivero *and* J. B. Boussingault *.

TO the south-west of Merida, and at one day's journey from this city, in the direction of La Grita, is a small Indian village, called Lagunillas, on account of its being situated at a short distance from a lake, whence the aborigines have extracted for a great number of years past a kind of salt called *Urao*.

The lake is about 3281 feet long and 820 wide. Its greatest depth is not quite ten feet. It is situated in a clayey soil containing large fragments of a secondary freestone. By a barometrical observation we have made, it appears to be 3323·65 feet above the level of the sea. In order to obtain the *urao*, the Indians make under the water an excavation of several yards in extent, in which they place a pole, from 14 to 16 feet long, the top of which projects above the water. This being done, an Indian leaning on the first pole directs another towards the bed of salt at a certain angle. On this pole a second Indian immediately slides down, dives, and after a few minutes spent under the water returns with some fragments of the salt. According to the information we have received, there is previous to reaching the *urao*, the bed of which is not very thick, about a yard of mud and a bed containing many crystals of carbonate of lime. The water of the lake is but slightly saline, and animals drink it with avidity.

The *urao* is crystallized in prismatic needles which appear to diverge from a common centre; it has a glassy aspect; its hardness is rather less than that of carbonate of lime; its taste alkaline, and similar to that of carbonate of soda; it does not effloresce on exposure to the air. We pass over the details of the analysis, and only give the results:

Carbonic acid	0·3900	Water	0·1880
Soda	0·4122	Foreign substances and loss	0·0098

This salt therefore contains more carbonic acid than the carbonate, and less than the bi-carbonate. The carbonate of soda called *Trona*, analysed by Klaproth, and which came from the province of Sukena, near Fezzan in Africa, has much analogy with the *urao*. The following are the results obtained from it by the chemist of Berlin,

Carbonic acid	0·3900
Soda	0·3800
Water	0·2300

* From *Annales de Chimie*, tom. xxix. p. 110; originally published in Spanish, at Bogota.

The

The *urao* salt is used in the country as a kind of mordant to an extract of tobacco, which, when held in the mouth, produces a secretion of saliva: this preparation is called *chimo* or *moo*. At Merida they mix four *arrobas* of *urao* with eight of tobacco; at Varinas the *urao* forms but one-fourth of the proportion of tobacco. The *moo* contains less *urao* than the *chimo*.

XLIV. Decas quinta novarum Plantarum Succulentarum; *Autore* A. H. Haworth, *Soc. Linn. Lond.* —*Soc. Horticult. Lond.*—*necnon Soc. Cæs. Nat. Cur. Mosc. Socio, &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

A FIFTH Decade of new Succulent Plants I have hereunder the pleasure of adding to those you have already been pleased to publish in your useful and scientific Magazine.

Some of these new plants are very remarkable ones, and they are all of the Aloëan family; all recently received from that inexhaustible mine of succulent plants the Cape of Good Hope; and were all detected there in truly native wilds, by that successful explorer of those arid regions Mr. Bowie; who safely transmitted them to our gracious sovereign's unrivaled gardens at Kew, where they are now flourishing, and scarcely seem to miss their warm and native skies.

Three of these plants with remote alternate sheathing leaves are said to have red, or yellow neat flowers; and one of these, which bloomed at Kew, is reported to have had porrected stamens; which character, combined with the remote and alternate habit of the leaves, may perhaps lead to the future construction of a new genus:—nevertheless, I consider the leaves of all Aloëan plants as actually alternate, although from their usually compacted aggregation seldom offering ocular demonstration of their being so. Those of many arborescent and other *Semperviva*, and likewise of the *Mesembryanthema capitata*, I know to be alternate; and they are quite as densely crowded, and after the same manner, prior to the production of flowers; as very gradually appears by their method of evolving their bracteate inflorescence.

I remain, sir, yours, &c.

Queen's Elm, Chelsea, Sept. 9, 1825. A. H. Haworth.

Classis et Ordo. Hexandria Monogynia.

Genus, Aloe *Linn.*, *Duval*, &c.—*Nob.* in Philosoph. Mag. Octob. 1824.

Sectio

Sectio, ARBORESCENTES, caule fruticoso simplici nec dichotomo, foliis angustioribus; propaginibus, si ullis, basilaribus.

gracilis. 1. A. (soft distant sword-leaved) foliis subdistantibus effusè incurvo-recurvulis angustè longéque lorato-acuminatis glaucis mollibus: marginalibus denticulis remotiusculis minutis.

Habitat C. B. S. ubi hanc et sequentes invenit assiduus Bowie.

Obs. Nunc tripedalis viget gracilisque *Aloën macram* aliquantum simulans. *Caudex* gracilis. *Folia* caulem vaginantia ut in *A. frutescente*, sed affiniora fortè ad *A. cæsiam*, at longiora, graciliora, et molliora tactu; etiam minùs crassa quàm in multis, marginalibus dentibus subroseis longè minoribus. Nondum floruit in regio horto. G. H. ♄.

Sectio, ACAULES, foliis multifariis ciliato-spinosis, propaginibus lateralibus sæpè cæspitosis.

subtuberculata. 2. A. (smooth and rough spotted) foliis glauco-cærulescentibus lævibus, subtus supernè maculato-tuberculatis, maculis infimis inelevatis.

Habitat C. B. S. G. H. ♃.

Obs. *A. acuminatæ* Nob. proxima, sed adhuc duplò minor, *foliis* magìs cærulescenti-glaucis albisve, spinis marginalibus numerosioribus potiúsve confertioribus, brevioribus niveis. Foliorum tubercula pauca, apicem versùs solùm posita, horum infima lævia et immersa, nec elevata, maculas veras læves formantia. *Flores* non examinavi.

aristata. 3. A. (bearded many-leaved) foliis lorato-acuminatis gracilibus numerosissimis, aristâ longâ finientibus.

Habitat C. B. S.

Floret Sept. pulcherrimè. G. H. ♃.

Obs. Species præsingularis atque formosa, magnitudine ferè *A. tuberculatæ* Nob. et pone id fortassè locanda, at *foliis* gracilioribus numerosioribus subincurvis expansis utrinque convexulis crebrè ac validè albo-serratis, aristâque semunciali (citiùs) emarcidâ: *suprà* glabris lævibus serie unicâ longitudinali centrali apicem versùs tuberculorum spinularumve: *subtus* albo subfasciatim tuberculatis spinulosisque, spinulis herbaceis innocuis affinium. *Flores* spicati formosi pallidè corallini, ferè ut in affinibus.

Sectio (nova) MACRIFOLIÆ, frutescentes graciles, foliis

foliis distantibus vaginantibus lorato-acuminatis macris angustis effuso-recurvulis, marginalibus dentibus semper minutissimis numerosissimis: floribus (ut audivi) luteis coccineisve, lateralibus.

Obs. Caules senecti (secundùm Dom. Bowie) in natalibus locis elongati longè decumbunt Ruborum fruticosorum modo, assurgentibus apicibus. Fortassè proprium genus, floribus mihi ignotis.

ciliaris. 4. A. (fringing broader*-leaved) foliis oblongo-lanceolatis brevibus, denticulis infimis sensim majoribus ciliiformibus.

Habitat C. B. S.

Floret . . . . . G. H. ♄.

Obs. Caudex nunc 3—5-pedalis apud Kew, gracilis erectus. *Folia* remota flaccida viridia lævia et fortè omnium minùs carnosa sive tenuissima.

striatula. 5. A. (stripe-sheathed narrow-leaved) foliis lorato-acuminatis angustis, vaginis pallidis viridi concinnè striatulis, spinulis marginalibus uniformibus.

Habitat C. B. S. G. H. ♄.

Obs. Fruticulus in regio horto nunc 3-pedalis, priore vix gracilior, foliis involuto-concavis duplò angustioribus glaucescentibus duplóque longioribus, marginalibus denticulis minoribus, parùm remotioribus. Paulò simulat *Aloën macram* Nob. at omninò tenuior, foliorum denticulis longè numerosioribus minoribusque. Adhuc in regio horto non floruit.

tenuior. 6. A. (green-sheathed narrow-leaved) foliis lorato-acuminatis vaginis viridibus, spinulis marginalibus minutissimis numerosissimis uniformibus; caudice gracillimo.

Habitat C. B. S. G. H. ♄.

Obs. *Folia* suprà subinvoluto-concava viridia, vaginis viridibus obsoletissimè striatulis cum saturatiore viridi. Ultimæ simillima, at caule duplò graciliore, foliis angustioribus denticulis marginalibus numerosioribus minoribus. *Folia* etiam aliquot simulant *Aloën macrocantham* Nob. at longè distantiora angustiora minora immaculataque. Semel florebat in regio horto Kewense, floribus (ut audivi) luteis, 6-7 andris, staminibus exsertis.

* Hujus sectionis.

Genus, GASTERIA, *Duval* in Cat. Pl. Succ. in Horto Alenconio. A. D. 1809. p. 6.—et *Nob.* in Synops. Succ. &c.—Aloë *Aliorum.*

Calyx petaloideus, curvus obclavatus, basi staminifer. *Capsula* parùm costata.

Fruticuli vix caulescentes, foliis Alöium; floribus pendulis. *Duval.* l. c.

Obs. *Scapi* semper remotè spathaceo-bracteolati, foliis linguiformibus, ensiformibusve.

Sectio, DISTICHA, Acaules: foliis distichis.

ensifolia. 7. G. (long sword-leaved) foliis elongatis angustè ensiformibus cultratis cuspidatis: confluenter multiguttatis.

Habitat C. B. S. G. H. ♃.

Obs. Acaulis foliis nunc exactè distichis bipedalibus; suprà planis obscurioribus, subtus parùm convexis, at macrioribus quàm in plurimis *Gasteriis*, guttis numerosissimis albis sæpè confluentibus marmoratis, potiúsve ferè tectis, præcipuè subtùs, ibique pulchrioribus albidioribus.

G. candicanti *Nob.* affinis, at foliis duplo ferè angustioribus, minùs confluenter maculatis, et adhuc omninò sine carinâ. Nondum in regio horto floruit.

Genus, HAWORTHIA, *Duval* in Cat. Pl. Succ. in Horto Alenconio. A. D. 1809.—et *Nob.* in Philosoph. Magaz. Octob. 1824.

Sectio, RETICULATÆ, foliis ambienter rosaceove-multifariis mollibus lævibus lætè viridibus integris vel obsoletè denticulatis; sæpè mucronato-aristatis; supernè ad lucem plùs minùs pellucidis reticulatisve: scapo simplici.

obtusa. 8. H. (small blunt hollow-leaved) foliis ovatis concavis luridis obtusis cum mucronulo, apicem versùs pellucentibus, lineis saturatioribus.

Habitat C. B. S. G. H. ♃.

Obs. H. concava *Nob.* in Revis. Succ. proxima, at subduplò minor, colore adhuc luridâ, et minùs virente glaucove quàm in illâ, foliis apice crassioribus obtusioribus magìs pellucentibus magísque striatis. Flores ut in affinibus.

planifolia. 9. H. (flat oval-leaved) foliis ovato-acuminatis suprà planis pallidè viridibus, senioribus reflexis.

Habitat

Habitat C. B. S. G. H. ♃.

Floret verno tempore, more affinium.

Obs. H. concavæ simillima habitu et magnitudine, foliis fortè minoribus, aristulâ longiore, paucioribus (sive adhuc nullis) propaginibus, uno singulari excepto in summo scapi floriferi, quæ in nostro exemplo per annum ultráve persistit. Distinguitur optimè foliorum planâ paginâ.

angustifolia. H. (slender-leaved) foliis lorato-attenuatis arcuatim patenti-recurvis perviridibus, apice subcarinatis; carinâ orisque creberrimè denticulatim exasperatis. 10.

Habitat C. B. S. G. H. ♃.

Floret Junio, affinium more.

Obs. H. chloracanthæ Nob. proxima, at minor, foliis multò angustioribus, patentioribus et longioribus s. 3—4-uncialibus, *subtùs* convexis, *suprà* planioribus, sæpiùs lineâ elevatâ longitudinali centrali; marginalibus denticulis minutissimis, et inarmato oculo vix conspicuis.

XLV. *On Mr.* Burns's *Method of finding the Latitude by Double Altitudes.* *By* Thomas Henderson, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN your Number for September, art. xxx. there is professed to be given a short, direct, and accurate method of finding the latitude at sea by double altitudes and the time between. But I believe that the following considerations will show, without difficulty, that this method is altogether inefficient, and produces erroneous results, which may often lead to serious consequences.

The author first assumes the trigonometrical formula for determining the altitude of a celestial body by means of the latitude, declination, and horary angle, on which the solution of this problem in the Requisite Tables depends, and which being transformed gives the well-known equation for expressing the cosine of a side of a spherical triangle in terms of the sines and cosines of the opposite angle and the other sides. This equation, which Delambre and other late writers make the foundation of all spherical trigonometry, might have been assumed at once, without being deduced from one of its own corollaries. The author next finds an equation for determining the latitude from the observed altitudes *and the times or horary angles*, which, he says, from the improved state of our

chronometers and other instruments may be known with the greatest exactness. But he must be aware that, if the horary angles can be ascertained at sea with such precision, two altitudes are not necessary for finding the latitude, one only with its horary angle being sufficient. It is well known, however, that it is impossible to determine, with the requisite accuracy, the times or horary angles at sea, independent of a knowledge of the latitude or longitude; and accordingly all the solutions of the problem of double altitudes hitherto given suppose only the difference of the horary angles, or the elapsed time, to be known. The serious errors produced upon the latitude from a defective knowledge of the horary angles will be seen by differentiating the author's equation, or even by considering the two examples which he has computed. For his final equation is identical with Dr. Brinkley's equation (3), which again is the algebraic expression of Douwes's rule for computing the middle time between the observations, or the horary angles from a supposed latitude by account. See Dr. Brinkley's paper, p. 14, annexed to the Nautical Almanac for 1822, where the coefficient 2 has, from a typographical error, been omitted in the denominator of the second member of the equation. The horary angles, which the author has assumed in his examples, being the approximate values found by Dr. Brinkley from the first operations by Douwes's method, when substituted in our author's formula they of course give for latitudes the quantities assumed by the Doctor for the latitudes by account. This is evidently going round in a circle; and yet it is from the results derived from this process that the method of solving this problem, proposed by one of our first mathematicians and astronomers, is asserted to give errors of one degree of latitude. In this manner it might be convicted of errors to any amount.

Navigators will always prefer short and easy methods of performing the astronomical problems requisite at sea, and therefore it is incumbent upon those who publish such methods to be well assured of their accuracy. It is to be regretted that our author had not taken the trouble of computing his examples in the direct trigonometrical manner; for then he would have at once discovered his error, and the censure, which he seems to pass upon the methods hitherto proposed (several of them by the most eminent mathematicians and astronomers of Europe), would have been spared.

I am, sir, yours, &c.

Leopold Place, Edinburgh,
Oct. 14, 1825.

THOMAS HENDERSON.

XLVI. *Ex-*

XLVI. *Examination of the Platina found in Russia. By M.* Laugier*.

A NOTE translated from the German Gazette of Petersburg (October 1823), relative to the voyage made by M. Soïmonof the senator and Dr. Fuchs to the Oural mountains, to examine the gold mines which have been discovered there, states that platina has also been found in them.

"I had made two years ago," says M. de Humboldt, in a letter which accompanied the specimens of which we are speaking, "fruitless attempts to procure this Russian platina: I have succeeded at last through the goodness of M. the Baron de Schilling. It is curious to see that the platina of the Oural is found in the midst of fragments of diorite (grunstein, or an intimate mixture of feldspar and amphibole), like the platina of Choco. The grains, which are rich in osmium and iridium, appear to me geologically interesting. At Choco each grain contains all the metals; in the Brazils alone, grains of palladium are found mixed with grains of platina, with grains of gold, and with diamonds," &c.

Two specimens of the ores of platina from Russia were transmitted to me by M. de Humboldt: one, of platina found in the auriferous sands of Kuschwa, 250 wersts from Ekaterinebourg; the other, in larger grains, considered as a combination of iridium and osmium, taken from the estate of the merchant Rastorgujers, in the Oural, near Ekaterinebourg.

The first, formed of very small laminæ of a grayish white, has the appearance of the platina of Choco, but is less bright, and more of the colour of lead.

The specimen weighed but 9 decigrammes: the magnet does not act on any one of the grains which compose it. Four decigrammes were perfectly dissolved in hot aqua-regia: there only remained some small white brilliant laminæ, hardly forming half a centigramme, or the 80th of the quantity submitted to the trial. The solution, evaporated nearly to dryness to drive away the excess of acid, was diluted with water and mixed with a quantity of hydrochlorate of ammonia sufficient to precipitate all the platina. The precipitate, of a fine dark-yellow, left, after washing and calcining, 0·27 of platina in the spongy state. The supernatant liquor not having yielded a triple salt of platina by evaporation, I diluted it with water and poured an excess of ammonia into it. The precipitate which resulted from it, washed and calcined, weighed 9 parts.

It easily dissolved in hydrochloric acid, excepting a very small quantity of a black powder, which would not dissolve

* From *Annales de Chimie*, tom. xxix. p. 289.

even

even in hydrochloro-nitric acid, and which I think is rhodium: but I could not discover the least trace of palladium. This metal, and the rhodium which generally accompanies it in ores of platina, hardly form the two-hundredth part of them. It is almost impossible to verify its existence when quantities so small as those given to me are experimented upon,—that is to say, on portions of a gramme.

In another experiment, 2 decigrammes of the same specimen gave me 0·13 of platina, 0·64 of oxide of iron, and traces of copper, of osmium, and of iridium, the presence of which it is easier to detect than that of palladium and of rhodium.

In these two trials of the platina of Siberia I obtained very nearly the same proportions; and I experienced every time a loss of nearly a seventh part of the quantity employed, the cause of which I cannot understand.

The second specimen weighed only 0·67. Its more complicated composition required a more careful examination. It is in general formed of grains of the size of large pins' heads a little flattened. It is to be remarked that these grains differ in colour: some are gray, others of a pure white; and others, smaller and of a blackish-gray colour, are separated by the magnet. These last formed but the tenth part of the specimen. The white grains compose nearly the third part of it; the gray are the most abundant. I took two decigrammes of the grains on which the magnet had no action, and I treated them with aqua-regia formed of the two concentrated acids, in the proportion of one part of nitric acid and two parts of hydro-chloric acid.

The first portion of acid was coloured brown, a second portion brownish-red, a third portion dissolved nothing more. There remained some small grains of a pure silver-white, weighing 5 centigrammes, or the quarter of the ore employed. This residuum, remarkable for its brightness, is so likewise for its hardness, and the force of cohesion which unites its particles. These two properties make it differ greatly in aspect from the residuum or black powder generally obtained from the platina of Peru which has been treated with aqua-regia. This last, in smaller grains, is easily reduced to powder and is easily attacked by nitre. The residuum of the platina of Siberia is very difficult to break by the hammer in flattening its grains a little, which supposes a little ductility; it cannot be pulverized, and the nitre does not seem to act upon it. Three successive treatments with nitre at a red heat neither diminished its hardness nor its weight. I was obliged to have recourse to caustic potash, and treat it three times in a silver crucible with three grammes of this alkali, to attack completely the five centigrammes upon which the aqua-regia had not acted.

The

The three masses obtained, steeped in water, disengaged a decided smell of osmium, and deposited a black powder. The alkaline solution, filtered and saturated by nitric acid, gave a smell of osmium; and there were formed light greenish flakes, which M. Vauquelin, in his excellent researches, ascertained to be a mixture of iridium and of titanium: the same solution becoming yellow as the saturation took place, indicated some traces of chrome. This solution, supersaturated with nitric acid and distilled, gave a white liquor having a strong smell of osmium, and acquiring a blue colour by the addition of some drops of tincture of galls. It is not to be doubted then that it was osmium; the black powder dissolved in the cold in weak hydrochloric acid, giving it a greenish colour which became red by heat. This solution lost its colour by the addition of some drops of protosulphate of iron. From these properties the presence of iridium cannot be mistaken.

Thus the portion insoluble in acids forms a quarter of the rough ore not attracted by the magnet; and I consider that it is composed (in five parts) of three parts of iridium and of one part and a half of osmium.

After the examination of the residuum insoluble in aqua-regia, I occupied myself in investigating the nature of the substances which this mixture of acids had dissolved. I evaporated the solution to dryness and dissolved the residuum in water, adding as much hydrochloric acid as was necessary to render the solution complete. I poured hydrochlorate of ammonia into the solution until it no longer formed a precipitate there. The precipitate having subsided, I decanted the supernatant liquor, which contained but a little iron, and which did not show any trace of palladium. The precipitate formed by the hydrochlorate of ammonia was of a chamois colour, which indicates a mixture in the platina of a small quantity of iridium. I dried and calcined it to obtain the platina from it; but instead of having it in the form of sponge or of metallic powder, there only remained a red-brown oxide similar to the oxide of iron. This residuum, of the weight of 15 centigrammes, dissolved indeed in the hydrochloric acid like this metallic oxide, and only left four parts of powder of platina soluble in aqua-regia, which was now precipitated wholly, and in a yellow triple salt, by solution of hydrochlorate of ammonia. The muriatic solution contained only iron, for the precipitate which the triple prussiate of potash formed in it was of the purest blue. The precipitation of this iron took place in the same time with that of a less quantity of platina by sal ammoniac, leaving me in some doubt of its purity. I precipitated it by an excess of ammonia, which preserved a very sensible blueish

blueish tinge. The ammonia, supersaturated by some drops of nitric acid and mixed with hydrocyanate of potash, gave a peach-bloom precipitate, which indicated that the blue colour which it had acquired was owing to a little copper.

The oxide of iron separated by ammonia was calcined with wax to produce the reduction of it, and in this state it was entirely attracted by the magnet: it weighed 10 centigrammes.

Thus it cannot be doubted but that this was iron in a state of perfect purity.

From this assay it results that the grains, considered as an alloy of iridium and of osmium, are formed, in 20 parts, of the substances hereafter designated.

Portion insoluble in aqua-regia, five parts; alloy of iridium and osmium, with traces of titanium and of chrome.

Portion soluble in aqua-regia, fifteen parts; of which ten are iron, four platina, half a centigramme of copper, and some traces of iridium.

The small quantity of the ore raised by the magnet, and which forms the tenth part, is composed of iron, of some atoms of platina, and of the alloy of iridium and of osmium.

The gray-coloured grains include more than a half of iron, a little platina, and some of the alloy of iridium and osmium.

As to the white grains, they appear to be almost entirely formed of the alloy of iridium and osmium, although they also contain a little platina and iron.

XLVII. *Analysis of an Urinary Calculus from a Hog.* By M. Wurzer, *of Marburg**.

THIS concretion had been cut in July 1824, at Fulda, from the urethra of an emasculated pig under a twelvemonth old. It had the size and shape of a small bird's egg. It weighed 91 grains (new med. weight). Its specific gravity at the temperature of 53° F. was 1·964. It was covered by a thin light-gray crust, and when broken presented a radiated and concentric crystallization.

1. A fragment heated before the blowpipe soon became brown, then black, and at last white again. During the process the ammonia, being extricated, became distinctly perceptible.

2. 100 parts of this substance, heated on a water bath until all the water had been expelled, lost 43·573.

3. The pulverized stone boiled with distilled water, the liquid poured off, filtered and evaporated, gave a residuum

* From Schweigger's *Journal*, Band xiii. p. 300.

of

of an urinary smell; and again dissolved, and the slimy matter being separated from it, it gave the following results with reagents.

a. With nitrate of silver it gave a precipitate immediately.

b. With nitrate of barytes it remained unchanged.

c. With solution of neutral muriate of platinum after the lapse of some hours, from its dilute state, the well-known reddish double-salt of platinum, and potash arose in the solution. After 24 hours it no longer increased.

According to repeated experiments I have made before (which thus deviate from those made by Wollaston and Marcet), 100 parts of this double-salt are equal to 29·6 of muriate of potash. As I obtained from 100 parts of this stone 8·868 of the double-salt, they indicated 2·625 of muriate of potash, of course taking the dissolved double-salt also into account.

4. Some of the powdered concretion heated with a solution of caustic alkali, filtered, and saturated with acetic acid, did not produce a deposit, nor did it become turbid; thus proving that the stone contained no phosphoric acid.

5. The residuum of No. 3, shaken with potash till the smell of ammonia had disappeared, filtered, saturated with muriatic acid, mixed with ammonium [?] to excess, and then with a solution of muriate of lime, gave a precipitate of phosphate of lime.

6. In powder this calculus was easily dissolved (and without evolution of carbonic acid) in dilute muriatic acid. By ammonia I precipitated triple phosphate of lime and ammonia, and by half-an-hour's ignition converted it into phosphate of lime.

7. From a solution of the calculus in muriatic acid, triple prussiate of potash immediately precipitated prussian blue. This concretion therefore contains iron in the state of protoxide.—It consisted of:

Phosphate of lime and ammonia . .	51·787
Muriate of potash	2·625
Protoxide of iron	0·169
Slimy matter with a urinous smell .	1·648
Water	43·573
	99·802
Loss	0·198

This concretion, although differing in form and other physical properties from the urinary calculus from a hog analysed by my meritorious friend Brandes, greatly resembles it in its component parts and their proportions. The only difference is, that the present subject of examination contains muriate of potash, (which, as far as I know, has not as yet been found in similar stones,) and protoxide of iron.

XLVIII. *On the Solution of various Kinds of Steel and Iron in Acids; and on the Nature of the Residuums left by them. By M.* KARSTEN*.

THE action of the acids on steel depends on the *degree of hardness* which the steel has received. Steel, hardened to the utmost of its capacity, is dissolved with very great difficulty and slowness in diluted acids. In diluted muriatic acid, it becomes covered after a few days with a black powder; and the solution makes so little progress within the space of several weeks, that it would seem to require many months before it is completed. White raw iron shows absolutely the same habitudes as steel, only that the effects are more striking. Diluted muriatic or sulphuric acids have scarcely any effect on this kind of iron, on which the black powder does not appear until after the expiration of several weeks. Strong muriatic acid, aided by a boiling heat, produces a solution without leaving any residuum. Sulphuric acid, under similar circumstances, leaves some carbon of a black colour and with a metallic appearance. Nitric acid at common temperatures separates black flakes, which by a longer exposure to the acid turn to a brownish-red. By boiling, a violent effervescence is produced, together with the other circumstances just mentioned.

Very different from this are the habitudes of gray raw iron to the acids. Diluted muriatic and sulphuric acids act but very slowly, and leave at the lapse of *several months* a residuum containing carbon in very different conditions. One part consists of very thin leaves or scales, of a perfectly metallic appearance and strong lustre. They resist the effects of acids and alkalies of every description, are not in the least attracted by the magnet, and are very slowly consumed in a red-hot platinum crucible. This body has been long known as graphite. Another part of the carbon has indeed also the graphitic appearance, but is subject to magnetic influence, and is in every respect like the residuum left by soft steel when treated with the acids. A third portion, in fine, has a black colour, is not magnetic, colours a caustic alkaline solution black, and is consumed before the crucible becomes ignited. Among these three bodies the graphite is never missing; whilst, of the two other combinations, one only generally appears in the residuums.

Strong muriatic acid produces a more rapid solution, rendered still more so by boiling. The hydrogen disengaged by it, mechanically carries up the graphite with it in the solu-

* From Schweigger's *Journal*, Band xiii. p. 335.

tion.

tion. The residuum contains carbon in a state different from the graphite; but gray raw iron can never be dissolved in muriatic acid without leaving it. Strong sulphuric acid, employed under similar circumstances, leaves, besides the graphite, black carbon easily combustible and not subject to the magnetic attraction.

Nitric acid of specific gravity 1·3 does not operate strongly on gray raw iron at common temperatures. At the same time effects are produced which appear to coincide, sometimes with those exhibited by hard steel, and sometimes with those characteristic of soft steel. The former appear in the darkest and softest and most ductile gray raw iron, and the latter in the lighter coloured and at the same time less soft and ductile kind of this metal. The action of the acid appears to be interrupted; for the solution seems to cease from time to time entirely, but re-appears with great violence on the breaking off of a leaf of graphite. The same appearance takes place in a boiling temperature; and the violent progress of the solution—which however only lasts for a few seconds—is combined with the falling off of a leaf of graphite: so that the graphite must actually form a mechanical impediment, protecting the iron against the attacks of the acid, and rendering the solution so difficult, that at common temperatures it is only affected in several weeks; and at a boiling heat, in several hours. The colouring of the acid proves that part of the carbon contained in the iron is dissolved: the residuum seldom consists of pure graphite, but mostly of graphite with more or less of carbon changed into a brown powder.

In order to explain these appearances attending the solution of different kinds of iron in acids, it is necessary to find out the nature of the substances which separate during the process of solution. The graphite being insoluble in acids and alkalies, may be stated to be quite pure. In a strong red-heat and exposed to the atmosphere it is volatilized slowly, without leaving any residuum. In order to volatilize 18 grains of graphite under the muffle of an assaying-furnace four hours were required, although the muffle was kept during the whole time at a white heat. This rather considerable quantity of graphite left on the platinum plate, on which it had been spread in order to give the heated air a greater scope for action, only a trace of silica, perfectly white, which had escaped the effect of the alkaline solution. In this process of combustion the graphite gradually decreases in bulk, and disappears at last without any kind of flame being perceptible. If the process be interrupted, the only difference seen between the part of the graphite calcined and that which is not, is, that the leaves

of the former, if held against the light, appear transparent in some places, and exhibit a peculiar fibrous structure, which the non-calcined graphite does not possess. Melted with nitre, the graphite produces no vehement detonations, but is consumed slowly, and the remaining salt dissolved without residuum by water. I did not succeed in converting sulphate into sulphite of potash by means of graphite. Thus then the graphite in gray raw iron is not what it has been supposed to be, a combination of carbon and iron; but pure carbon, or its metallic base. Whether natural graphite be also a pure carbon-metal, or really a combination of carbon and iron, is yet to be determined.

XLIX. *Extract from a Letter addressed by Professor* BESSEL *to Professor* SCHUMACHER, *relating to the Greenwich Observations*, as criticized in the Philosophical Magazine for November and December* 1824.

WHEN I had the pleasure of being your guest at Altona, you showed me the numbers of the Philosophical Magazine which contain a very severe censure of the Greenwich observations for 1821. I saw this censure with some surprise, because I had always considered the collection of observations at Greenwich as singularly valuable, and as a rich source of astronomical truths; nor were you, I believe, of a different opinion; and we were perfectly agreed respecting the unimportance of the inaccuracies that were imputed to this work in the two papers published in the 64th volume of the Philosophical Magazine.

For those who are acquainted with the Greenwich observations, and who compare them with the critic's remarks, every further explanation would be superfluous. But since it may be supposed that these remarks will fall into the hands of many persons not deeply versed in astronomy, I readily comply with the request which you made, that I would commit to writing our common view of the subject. I feel, as well as yourself, the propriety of doing my best on the occasion, in order that too great importance may not be attached to this censure of an establishment, to which astronomy is indebted for a great proportion of its advancement; and that its importance cannot be very great, is sufficiently shown by the facility with which Mr. Olufsen has computed the declinations of the fundamental stars, as published in the *Nachrichten*, No. 73, from the Greenwich observations for 1822.

* From the Astronomical and Nautical Collections in the Journal of Science, &c. vol. xx. p. 108.—The passages within brackets are as given in the Collections.

The

The greater number of the errors which have been pointed out by the censor are merely accidental errors of the pen. Errors of this kind are certainly disagreeable, and it would be better if they could be entirely avoided; but since all collections of observations in existence do contain such errors, they clearly appear to be unavoidable.

The *first* class of errors mentioned in the Philosophical Magazine contains the cases in which the mean deduced from the readings of the two microscopes A and B differs from the column in which that mean is assigned. Since there must be some manifest oversight in all these cases, it may sometimes be difficult to determine whether it is in the readings or in the mean assigned; but it will, in general, be easy to distinguish, from the preceding or following observations of the same star, where the error lies.

The *second* class contains the differences between different records of the same observation. These must be errors in the copies sent to the press, and not in the readings of the microscopes; and they may generally be corrected by a comparison of the two passages: they sometimes extend to whole degrees, or to the tens of the minutes, and are then of no importance; for example, in the observations of *Procyon* the 23d February 1821, and of β *Cephei* the 8th December, where there are errors of 30° and 5° respectively.

The *sixth* class of errors contains the intervals between the micrometer wires, as they are deduced from different observations of the same star. These are often dependent on errors of the pen, as in the observation of *Capella* on the 7th February, and in that of *Sirius* on the 8th, where there are errors of $5''$ and of $40''$ respectively in the fourth wire: frequently also they arise from inaccuracies of observation. In the former case they are of no consequence whatever, being easily detected at first sight; in the latter they are fundamental imperfections; but such imperfections are inseparable from the nature of observations, and it would be ridiculous to expect from an astronomer that he should perform impossibilities, *All* registers of observations exhibit inaccuracies of this kind; and if any should be produced without them, it might with confidence be asserted to be a forgery. The diligence of the astronomer is proved, not by the perfect agreement in his tenths of seconds, but by the magnitude of his mean or his probable error; and it would probably be difficult for the critic to prove that this error is much greater in the Greenwich observations than the nature of the instruments renders unavoidable.

The errors of the *fifth* class, which comprehends the differences between the polar distances observed with two and with

six

six microscopes, seem to me to have been introduced without the least propriety: they are either insignificant errors of the pen, as in the case of γ *Draconis* 28th March; or slight accidental errors of observation, mixed with the changes of place of the stars and of the refraction; or, lastly, changes of the place of the pole on the instrument. For this last the observer can by no means be responsible. Had the critic pointed out any new method of fixing the instrument so that it should be subject to no alterations, he would have deserved the thanks of all practical astronomers; but the constant result of past experience shows that the greatest possible care, in procuring a firm foundation for the pillars, affords us only a comparative and not an absolute stability. The fixing of the instruments at Greenwich has been such as to keep them for a long time admirably firm; but at other times it has not been so successful, as may be seen in the table of the place of the pole, printed in the *Nachrichten*, No. 73; the differences between the latter days of July and the beginning of August 1821 depending on a change of this kind, so that they cannot be considered as accidental errors of observation; nor are they of material importance, as they may be readily determined by a series of observations of the pole star, so complete as those which are made at Greenwich. The accidental irregularities of the polar distances, which remain after the correction of the place of the pole, can be as little considered as an imputation on the accuracy of the observer, as those of the intervals of the micrometer wires. The truth of this remark is illustrated in the *Nachrichten*, No. 73.

The *fourth* class contains the differences between the times of transits observed with the transit telescope, and the mural circle. The latter instrument, however, not being intended for the observation of transits, nor being ever actually so employed, it would have been of no manner of use to seek for greater accuracy in the memorandums which are made merely with a view of determining its place with respect to the meridian. We ought to acknowledge the occasional insertion of these memorandums with gratitude, as they assure us that the instrument never deviates so much from the meridian as to affect the polar distances; but they are not intended for any other purpose. Neither Bradley nor Maskelyne have ever noted the times of the transits by their mural quadrant, although it was more liable to variation than the mural circle. But to correct the place of the axis of this circle continually, so as to bring it perfectly into the plane of the meridian, would certainly be of no advantage to the Greenwich observations.

Other errors which are criticized,—for example, those of the names

names of the stars, of the hour or minute of their transits, and so forth,—are of no material importance whatever; and how difficult it is to avoid errors of this kind, may be inferred from the circumstance of my having found about 1400 such errors in Bradley's observations. [The catalogue of these errors is already printed at the expense of the Board of Longitude, and is to be annexed to the publication of Mayer's original observations, which is nearly completed.]

The remark, that the observations at Greenwich are commonly concluded at midnight, would be of some weight, if it could be proved that any thing essential is omitted by this practice, which does not appear to me to be the case. The observations relate chiefly to the sun, the fundamental stars, the moon, and the oppositions of the planets; and it may easily be discovered that these different series are exhibited with an uncommon degree of perfection. Had the censor in the Philosophical Magazine pointed out any other series of observations which could have been combined with these, so as not to interfere with them, no doubt the Astronomer Royal would have been much obliged to him. Every thing cannot be done at once in an observatory; and if as much is effected as can be wished in one respect, something must be omitted in others. But to multiply observations, without any plan or object whatever, would be mere idleness. *Whoever is dissatisfied with the actual riches of the Greenwich observations would do well to make the attempt to excel them; he would convince himself by such an experiment that the labour and patience required for doing so much are fully sufficient to exhaust the powers of any one man.*

The *third* class of errors, relating to the meteorological instruments, I have not yet mentioned, because I think myself that greater accuracy is required in this department than it has hitherto been usual to observe. And if I should be allowed to suggest any improvement that could be made in the observations at Greenwich, it would be a more correct account of the meteorological instruments, and of the place in which the exterior thermometer is fixed. [It may, indeed, be expected with confidence that Professor Bessel's desire to possess a barometer and a thermometer, correctly compared with those which are employed at Greenwich, will not long be allowed to remain ungratified, though it would be a subject of much surprise on this side of the Channel if he should detect in them such discordances as he is inclined to suspect.]

[This letter has probably appeared in Professor Schumacher's *Nachrichten*, though the 84th number of that interesting collection, for which it was intended, has not yet reached this country.]

L. *No-*

L. *Notices respecting New Books.*

Antediluvian Phytology, illustrated by a collection of the Fossil Remains of Plants peculiar to the Coal Formations of Great Britain. By Edmund Tyrell Artis, F.S.A. & G.S. London, 1825. 4to. Introduction xiii. p.: pp. 24: plates 24.

PRIOR to the appearance of this publication, we believe, the only English works in which the vegetable remains so abundant in our coal strata, and in many instances so interesting to the botanist as well as to the geologist, were attempted to be illustrated with any precision and detail, were Martin's *Petrificata Derbiensia*, and the late Mr. Parkinson's Organic Remains. Some of them had also been described by Mr. Steinhauer, in the American Philosophical Transactions; and a few others in the Transactions of various scientific bodies, in two or three county histories, and in the philosophical journals. The late researches of MM. Schlotheim and Sternberg, with the memoirs of Professor Martius and of M. Adolphe Brongniart, have given the subject a new and more scientific form than it has hitherto possessed; though we are somewhat apprehensive that those naturalists have not been sufficiently cautious in their determinations of genera and species, so little of the organization of the plants being in their present state capable of being correctly ascertained. We are not fully satisfied, moreover, with the principles of classification which they have adopted, as they appear to tend to the production of some confusion and anomalies in the established modes of discriminating the subjects of natural history.

On the whole, however,—as we have just intimated,—the investigation of the fossil remains of vegetables is rising in accuracy and usefulness; and though we might perhaps have wished for a work upon the subject on a scale that would place it within the compass of geological students in general, yet we hail the publication of "Antediluvian Phytology" with pleasure, as the first work in our language exclusively devoted to this subject.

In a brief introduction, Mr. Artis explains the motives and circumstances which have led him to the undertaking, and gives an outline of the respective systems of fossil vegetables and their parts, proposed by the naturalists we have named above; reserving an outline of a new arrangement, with various observations on the fossil plants of the coal formation, for another volume, or rather part, of his work, which he states to be already in progress.

The part before us contains twenty-four plates, well engraved by Weddell, from drawings chiefly by Mr. Curtis, but some

some by the author himself: each plate is accompanied by a page of descriptive matter; and we shall give the most correct view of Mr. A.'s labours, by extracting the generic and specific characters with the synonyms and localities, of the fossils he describes. We shall give one of his descriptions entire; and where not otherwise expressed, the genera and species in the following enumeration are new.

Hydatica. *Stem* arborescent, jointed, branched; *leaves* long, linear.

prostrata. Stem jointed, slightly striated; joints formed with irregular sutures, from whence arise tufts of linear leaves. —Found imbedded, in a compressed state, in the shale which forms the roof of the coal bed in the upper El-se-car coal-mine, near Wentworth, in the west riding of Yorkshire, the property of Earl Fitzwilliam: fragments of it may also be observed in the roofs of several chambers of that colliery whence the coal has been extracted, and which are called by the colliers "Old Binks."

columnaris. Stem branched all the way up, ending in a club-like head; branches alternate, simple, covered with leaves; leaves hair-like, parallel, two-ranked.—Found imbedded horizontally in the black micaceous shale which covers the thick coal in the upper El-se-car colliery.

Calamites, auct. *Stem* jointed, longitudinally striated; *impressions* at the articulations usually forming rings round the trunk.

ramosus. Stem arborescent, branched; branches cylindrical, inserted at the articulations of the trunk, striated; articulation of the branches surrounded by a striated disk.—Found imbedded both horizontally and vertically, in sandstone, in Lea-brook quarry, near Wentworth: also of a great length in El-se-car new colliery.

approximatus. Stem arborescent, jointed; joints very short, intercepted by distinct articulations, with small compressed tubercles, forming a studded ring round the trunk.—*C. approximatus*, Sternberg; *C. approximatus* and *C. interruptus*, Schlotheim.—Found imbedded horizontally in the soft sandstone at the bottom of the rock in Hober quarry near Wentworth, in other parts of Yorkshire, also in Durham, and near Newcastle; and on the continent.

Pseudo-bambusia. Articulations more or less distant according to the size of the plant; striæ intercepted at the articulations. *C. pseudo-bambusia*, Sternberg; *Phytolithus (arundineus) graminis?* Martin.—Of very common occurrence throughout the whole of the coal-formations of England.

dubius. Striæ narrow, with a fine obscure groove running

down their middle; the fifth or sixth articulation surrounded by a double line of impressions.—Found in the sandstone of Lea-brook quarry, near Wentworth, in Yorkshire.

decoratus. Stem arborescent; joints short, decreasing in length towards the summit, where they form an enlarged blunt head; striæ tuberculated at bottom close to the articulation; those of the head very broad. *Phytolithus sulcatus*, Amer. Phil. Trans. [Steinhauer?]—Found imbedded both horizontally and upright, in the Lea-brook quarry, Yorkshire, and in many other places; as also frequently in ironstone.

Ficoidites. *Stem* with cicatrices; *cicatrices* distant, depressed, having a tubercle in the centre surrounded by a hollow; *tubercles* bearing leaves or spines.

furcatus. Cicatrices approximate, nearly of equal size; spines long, linear, forked.—Found imbedded in the shale of the El-se-car new colliery, in a horizontal and compressed state.

verrucosus. Tubercles of two sizes, surrounded by a hollow: leaves or spines jointed, forked.—*Phytolithus verrucosus*, Martin, Amer. Phil. Trans. [Steinhauer?]—Found in the clay-bind incumbent on the sandstone, within a few feet of the surface: traces of it are also found on the upper side of the nine feet coal in the El-se-car mine.

major. Stem with a spike up the centre; cicatrices distant, oval sides compressed against the tubercles; tubercles oval at the base with a longitudinal furrow at top.—Found in a sandstone quarry near Rotherham in Yorkshire, as also at Crudling near Stanley, in the same county.

Filicites, auct. *Leaf*, or *frond*, forming a flat surface, the two sides of which are symmetric; secondary rib simple or forked.

Osmundæ. Leaflets broad, rather falcate; rib single, branched; branches divergent, forked.—Found in shale in El-se-car new colliery, and detached leaflets often occur in nodules of ironstone.

trifoliatus. Leaf or frond tripinnate; pinnæ or lobes alternate, with an odd one; leaflets ternate, lobes roundish, convex.—Subgen. *Sphenopteris*, A. Brongniart.—Found in the shale of El-se-car new colliery, in that part of the mine which is near Milton Furnace, but is not common.

Miltoni. Leaf or frond tripinnate; leaflets adherent to the rachis; fructifications in lines near the margin.—Same locality as the last species.

plumosus. Frond or leaf tripinnate, with an odd one; stipes wavy; leaflets lanceolate, sessile.—Found imbedded in the shale of El-se-car new colliery, in that part only which is situated

situated immediately under the reservoir: does not appear to have been found in our coal-formations elsewhere.

decurrens. Leaf or frond tri- or quadri-pinnate; leaflets linear-lanceolate, first leaflets decurrent; ribs pinnate.—Found in great abundance in the shale at Alverthorpe near Wakefield; also in the coal-mines belonging to the marchioness of Hertford, near Leeds.

Sternbergia. *Stem* with double longitudinal keels, terminating at different heights spirally round the stem, and furnished at their termination with small tubercles.

transversa. Stem ringed transversely; rings mostly distinct, sometimes uniting two or more together.—Found lying horizontally, accompanied with *Calamites ramosus* and *C. pseudobambusia*, in the clay-bind which alternates with the sandstone of Lea-brook quarry.

Rhytidolepis, Sternberg. *Stem* simple, furrowed; *furrows* wavy, impressed with dots or simple lines placed on the ridge.

fibrosa. Cicatrices ovate, subpentagonal, with a single gland; surfaces of the ridges fibrous.—Found vertical in the sandstone of a quarry at Rowmarsh near Rotherham, in Yorkshire; and a congeneric fossil very nearly allied to it in El-se-car.

Myriophyllites, Sternberg. *Stem* herbaceous, slender; *leaves* numerous, linear, small.

gracilis. Stem terminating in a sharp point, the whole thickly covered with hair-like leaves.—Found in El-se-car.

Euphorbites vulgaris. *Common Euphorbites.*

Gen. Char.—*Stem* arborescent, simple, subconical, furrowed; *cicatrices* on the ridge, generally nicked.

Spec. Char.—Euphorbites *vulgaris.* *Cicatrices* flat, fish-shaped, the upper part trigonal; *glands* two, which when the bark is absent appear as twin tubercles on the ligneous fibres.

Synonyms.—This fossil plant does not appear to have been described or figured by any author.

Description and locality.—*Stem* attains the length of nine feet, very wide at bottom, and narrower at the upper extremity, furrowed.

Furrows at the upper end narrow, but at the lower end much expanded, as shown in the lower figure: ridges of the upper extremity pipe-like, parted by a simple line; but those of the lower extremity wide, flat, and parted by a groove of equal breadth.

Cicatrices on the bark flat, resembling fishes, the upper part three-sided, the upper angle forked; the lower rounded; glands two, towards the upper part of the cicatrix. When the bark is absent, the woody part presents a fibrous appear-

ance, and these glands remaining form twin tubercles, close together in the upper part of the stem, but in the lower part distant, although the cicatrices appear to have been nearer together in the longitudinal direction. There is also a small dot on the cicatrix between the two glands, which is but superficial, and does not appear on the woody part of the stem.

Pith slender, passes up one side of the trunk.

Found in the greatest part of the coal-formations of Europe: either in immense masses incumbent on the coal, or in a vertical position, in which position its lower extremity frequently rests on the thin shale which covers the coal.

Observations.—The specimen from which the drawing was made is now in the possession of the Rev. Samuel Sharp, vicar of Wakefield, who procured it from a sandstone quarry near Altofts in Yorkshire, the property of Sir Edward Dodsworth, who has also one of the same species in his garden, where it forms one side of the entrance to a grotto.

In one of the abandoned chambers of the upper El-se-car coal mine, seven trunks of this plant were suspended freely from the roof; some of them projected a foot, and the largest measured eight feet in circumference.

The specimen figured is nine feet long, five feet in circumference at the lower extremity, and only one foot nine inches at the upper.

The first figure represents a sketch of the whole trunk, and shows also the situation of the pith. The second figure represents a portion of the upper part, of the natural size, in which is shown the different appearance of the bark and unbarked surface. The third figure shows a portion of the lower extremity in which the deep concave furrow, the flattened ridge, the twin glands placed at a greater distance apart in the horizontal direction, but much nearer in the longitudinal, are exhibited.

Aphyllum. *Stem* arborescent, covered with fleshy scales, inserted in hollows furnished with glands.

cristatum. Scales obovate, with an oblong crest in their centre; interstices forming angular furrows.—Found in a sandstone quarry at Banktop, Yorkshire.

asperum. Stem tapering; scales long, rhomboidal, those in the lower part of the stem separated by a considerable interstice resembling bark.—Found imbedded obliquely in shale, near a rent in the strata, in a continuation of the El-se-car nine feet coal, situated near Hoyland in Yorkshire.

Lychnophorites, Martius.—*Stem* branched, covered with tubercles; *tubercles* leaf-bearing; *leaves* narrow.

superus. Tubercles terminating obtusely at top; cicatrix quadran-

quadrangular at the upper extremity of the tubercle, gland-bearing; gland central; mid-rib of the leaf forming a continuation of the longitudinal ridge on the back of the tubercle. —Found in one of the sandstone quarries on Swinton Common, near Rotherham, in Yorkshire.

MEGAPHYTON. *Stem* arborescent, simple, furrowed longitudinally; cicatrices in the furrow between the ridges.

frondosum. Cicatrices near together, horse-shoe shaped with the points upwards, filling the furrow.—Found imbedded obliquely in the sandstone of a quarry near Rowmarsh in Yorkshire.

CARPOLITHUS, auct. The *fruits* or seeds of plants, in a fossil state.

marginatus. An ovate nut, rather bulged towards the top; edges broad, flattened; the tip blunt, slightly indented; *shell* deeply furrowed longitudinally.—Found in the fine sandstone of Lea-brook quarry.

In concluding our notice of this useful work, we would suggest to the author the propriety of giving, in future, his generic and specific characters and descriptions in Latin as well as in English; as we think this would add much to the utility of the publication, the second part of which we hope soon to have the pleasure of analysing.

Icones Fossilium Sectiles. Centuria Prima. London, 1825. Folio, pp. 4. Plates 8.

The most appropriate account we can give of this publication, and one that will show it to be worthy the attention of all who are interested in the study of organic remains, will be to quote the Latin preface, written with an elegance seldom found in modern scientific works in that language; and to give the descriptions of some new genera, as specimens of the manner in which the subjects are described. It is understood to be from the pen as well as the pencil of Mr. König, mineralogist to the British Museum.

"Magnam ex corporum quondam organicorum jam vero FOSSILIUM cognitione utilitatem capere Geologiam ad telluris stratorum naturam ætatesque rite intelligendas atque dignoscendas, inter omnes quorum de his rebus judicium est satis constat. Absoluta vero harum reliquiarum cognitio ex meris descriptionibus, etiamsi bonæ sint atque uberrimæ, vix ac ne vix quidem comparari potest. Quamobrem maximas scientia gratias debet viris egregiis, Brocchio, Brongniarto, Knorrio, Lamarckio, Parkinsono, Schlothemio, Sowerbæis, Sternbergio aliisque, ob navatam figuris accuratioribus evulgandis operam. Neque negandum tamen, in tanta supellectilis abundantia,

Promptuarium,

Promptuarium, omnium quantum poterit specierum figuris condendis dicatum, vehementer adhuc desiderari. Quæ cum ita sint, non perperam facturi nobis videmur si tali operi manus admovere conabimur. Etenim multæ fossilium icones in libris extant quibus potiri non cuique facile est, nec deficiunt species figuris omnino carentes. Ex quorum alteris eas, quæ ad unguem depictæ censebuntur, nova hîc luce donare, alteras vero ad naturam ipsam delineandas curare, constitutum habemus. Quæ vero paucula de instituti ratione exponenda videntur hæc sunt. Icones rudi quidem sed satis exacto calamo, lithographice (ut dicunt) delineatas, in tabulis confertim et nullo ordine ita tamen collocatas invenies, ut, per lineas longitudinales transversalesque descriptæ, distinctæ omnes appareant: hinc illas (quod in titulo exprimere tentavimus) disseces, separatasque in ordines atque genera disponas, licebit. Quas vero, in operis decursu, hinc illincque minus accuratas deprehenderis figuras (quod quo longius progredimur eo rarius futurum spes est) easdem, curis secundis subjectas, et quidem gratuito suppeditare non gravabimur.

"De tabulis hactenus: ad explicationem vero earum quod attinet, tantum adjiciemus quantum vel ad nova genera designanda, vel ad nominum mutandorum caussas exponendas, vel ad loca specierum natalia indicanda, necessarium videbitur; reservatis ceteris in aliud opus ad quod materiem jam colligimus, quodque, si tempus vacat, futuro die in lucem emittere propositum habemus.—Cuilibet animalium generi additus est locus quem tenet in methodo qua in opere cui titulus '*Regne animal*,' usus est Illustris Cuvier, appositis notis ad distinguendas divisiones; sic Ixæ nomen excipit phrasis—*Articulata. Crustacea: decapoda; brachyura, Canceres*—ubi punctum indicat *sectionum primariam*, colon *classem*, semicolon *ordinem*, comma *familiam*; quinto divisionis membro, *tribu* scilicet, absque nota relicto.—Quod reliquum est lectores instituto nostro faventes rogatos volumus, ut figuras aut observationes si quas habeant communicandas, mandare velint ad Georgium B. Sowerby*, illas lubenter recepturum."

Leucopthalmus, nob. (*Mollusca. Acephala: nuda; Ascidiæ,*)

Corpus globosum, coriaceum, pedicellatum. Aperturæ duæ pentagonæ, 5-radiatæ.

Quamquam mollusci structura interior in statu lapideo distingui nequeat, satis superque tamen hoc genus a cognato, Boltonia, differre videtur aperturis 5-radiatis, ut alias jam taceamus externæ structuræ differentias.

Nomen Leucophthalmi, quo usi sunt veteres ad notan-

* Londini in vico Regent-street.

dum

dum lapidem nec Plinio nec nostræ ætatis scriptoribus satis cognitum, sed inter sexcentas forte Achatis varietates quærendum, ad scientiam revocare novoque huic generi imponere non hæsitavimus.

LEUCOPHTHALMUS *Strangwaysii.* n.

Ex calcario argillaceo ripæ occidentalis Ligovcæ, in territorio Petropolitano, propter locum Crasnoi-Celo dictum. (Videsis egregiam illius territorii descriptionem geognosticam ab Honorabili Thoma H. F. Strangways datam in *Act. Soc. Geol. vol.* v.)

ASPIDISCUS, n. (*Polypi; corticati, Lithophyta*)

Polyparium orbiculare, superne convexum, cristis crenulatis, inæqualibus, decussantibus s. a peripheria ad centrum vergentibus munitum; inferne planum, striis circularibus concentricis notatum.

ASPIDISCUS *Shavvii.* n. (CYCLOLITES *cristatus* Lam. ?)

Nomen genericum a forma, specificum dedimus in honorem peregrinatoris celeberrimi, Thomæ Shaw, qui, quo tempore a sacris erat consulibus Anglicis apud Algerinos, exemplaria Madreporæ hujus in agro Tingitano collecta (nunc vero in Museo Britannico asservata) ad Illustrem Sloanium perferenda curavit.

LAMPETIA, nob. [PHAETHUSA, in tabula.] (*Dicotyledoneæ: Terebintaceæ ?*)

Capsula bivalvis, valvæ marginibus incrassatis, septiferæ: dissepimenta medio valvarum adnata, superne emarginata.

Cum sero intellexerimus, PHAETHUSÆ nomen, in tabula usurpatum, generi ex Compositarum ordine jam inditum esse, alterum alterius Heliadum nomen in ejus locum hîc substituimus. Huic generi species subjecta est unica:

LAMPETIA *lacrymabunda.* n.

Inveniuntur fructus cum ligno in marga arenosa et limosa alluvio comportata, in Prussiæ ora maritima, præcipue in litore Sudavico, ubi succinum (electrum) effoditur, quod ex hoc arbore defluxisse extra omnem dubitationis aleam positum videtur.

TRIGONOTRETA, nob. (*Mollusca. Brachyopoda: Terebratulæ,*)

Testa inæquivalvis, plana aut lobata; valvarum altera superne producta in rostrum externe convexum, interne planum, perforatum, subtriangulare; cardo infra foramen, plerumque linearis, strictus.

Processus flexuosi aut varie spiræ in modum convoluti qui, diruptis horum conchyliorum testis, aliquando in conspectum cadunt, minus sunt idonei a quibus notæ genericæ depromantur; nihil enim illis inconstantius, nec forma magis varium. Quamobrem illud Terebratularum genus cui a Sowerbæo

patre

patre nomen SPIRIFERI inditum est, Trigonotretis nostris adnumerandum erit. In plerisque illius generis speciebus (v. g. in S. *resupinato*, *cuspidato*, *minimo*) corpus spiriforme omnino desideratur; nec desunt Terebratulæ (proprie sic dictæ) quæ hujusmodi appendicibus munitæ videntur. Neque multum discrepat a Trigonotretis aliud ejusdem auctoris genus nomine PENTAMERI insignitum, de quo tunc agemus cum ad illius specierum figuras pervenerimus.

TRIGONOTRETA *Stokesii*. n.

Ex insula Van Diemen Novæ Hollandiæ. In transitionis (?) arenario.

A supra laudato Carolo Stokes nobiscum communicata.

TRIGONOTRETA *speciosa* (Terebratulites speciosus *Schloth.* excluso tamen, ut nobis videtur, synonymo *Leonh. Taschenb.* vol. vii. t. 2. f. 9.)

Ex Eiffelia.

The remaining new genera described and depicted in the work are *Trigonosemus* (*Mollusca. Brachyopoda, Terebratulæ,*); *Teratichthys* (*Pisces*); *Pharetrium* (*Mollusca. Pteropoda?*); *Spongus* (*Polypi. Ordo incertus*); and *Homalonotus* (Ex crustaceorum ordine cui nomen *Trilobitæ?*).

We hope soon to see Centuria II. of these *Icones fossilium sectiles.*

LI. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

June 8.—M. DEYEUX gave a verbal report on the *Manuel à l'usage des pharmaciens* by MM. Chevalier and Idt.—A note from M. Gambart was read, containing the parabolic elements of the last comet discovered by him.—The Academy being informed by M. Arago that he now has at Paris two living camelions, appointed a committee which they requested to make experiments on the changes of colour which the skin of these animals undergoes.—M. Giuseppe Cerini, an engineer of Milan, addressed a letter respecting a new machine for raising water for the irrigation of lands.—Mr. D. Barry, staff-surgeon in the English service, read a memoir on the motion of the blood in the veins.

June 13.—The Academy received a memoir, in manuscript, on the source of the heat of the thermal waters of Aix, by M. Robert; and some notes on the quarries of Montabusard, and the gravel-pit of Montchêne, near Chevilly, by M. Thion.—M. Bosc gave a report on M. Duvau's new researches on the natural history of Aphides.—M. G. Libri, of Florence, laid

laid on the table a memoir, in manuscript, on the theory of numbers.—A committee reported upon M. Brisson's memoir on analysis, which they thought should be favourably received, on account of the elegance of the method given by the author, and the importance of the objects to which it was applicable. —M. Fresnel read a note relative to the repulsion of heated bodies.—M. Magendie, in the name of a committee, presented the history of a person born deaf and dumb, to whom Dr. Deleau had given the faculties of speech and hearing by the catheterism of the larynx.—M. de Humboldt read a memoir, entitled A sketch of a geognostic view of South America; and he presented another, containing a thorough examination of the observations which have been made in various countries on the diurnal period of the barometer.

June 20.—A memoir was read, on the treatment of the ligature of some species of aneurism, for which the amputation of the members is commonly resorted to; by M. Dupuytren.

June 27.—M. Zugenbuhler claimed, by letter, the priority of the ideas stated in Mr. Barry's paper above mentioned, transmitting a copy of a dissertation printed by him six years ago, and entitled *Dissertatio de motu sanguinis per venas.*—M. Frizon laid on the table a manuscript respecting the summation of the similar powers of the roots of an equation.—M. Puissant transmitted a memoir on the determination of the figure of the earth by geodesic and astronomical measurements.—M. Navier made a report on M. Raynalt's memoir respecting a moving power of great force applicable to machinery of every description. It appears that the author has made bad choice of his data, and been deceived by some old experiments on the dilatation of the gases.—M. Vauquelin read a memoir on the existence of iodine in the mineral kingdom*. M. Girard read a memoir on the mutual attraction of two moistened surfaces, placed at sensible distances in the liquid in which they are immersed.—M. Adrien de Jussieu read a memoir on the group of *Rutaceæ*.

July 4.—Baron Blein transmitted a note on some experiments in acoustics, with the four bars of steel with which they had been made.—M. Thenard gave a verbal account of a work by M. Longchamp on the waters of Vichy.—M. Arago announced that Professor Kupfer, of Cazan, proposed to make a journey in Siberia, for the purpose of investigating the magnetic phænomena; and that he would undertake any experiments with which he might be charged by the Academy.—M. Girard†, director of the veterinary college of Alfort, read

* See the present Number, p. 269.

† We regret to hear that the fate of the celebrated Bichat has during the present month befallen M. Girard.—Edit.

a memoir on the herniæ of the horse.—M. Costa read a memoir on the yellow fever of Barcelona.—M. Bussy read a memoir on the action of heat upon the fatty bodies.

July 11.—M. Gaëtano Giorgini, formerly a student of the Polytechnic School, communicated a memoir on the causes of the insalubrity of the air in the vicinity of sea marshes.—M. Le Jeune d'Yrichlet transmitted a memoir on the impossibility of some indeterminate equations of the fifth degree.—M. Cagnon, a physician at Vitry-le-Français, transmitted a memoir, entitled Some reflections on the means of avoiding the rupture of the rectum, and of stopping the hæmorrhages that take place during or soon after the operation for the stone.—M. Arago communicated some new observations on the temperature of artesian springs, which entirely confirm the results formerly obtained. The spring of St. Venant maintains throughout the year a temperature of above 57° F., in a country the mean temperature of which certainly does not exceed 51°·8 F. This spring rises from the depth of 328 feet.—M. Arago also presented the first table of the meteorological observations which will in future be made at the Tower of Cordouan.—M. Lasserre read, in his own name and that of M. Costa, some explanations of the proposition which he had made at the preceding sitting. In order to prove that the yellow fever is not contagious, MM. Lassis, Costa, and Lasserre offer to put on the clothes that were worn by persons who have fallen victims to that disorder.—M. Nicollet read a memoir, by himself and Col. Brousseaud, entitled An exposition of the operations relative to the measure of an arc of mean latitude between the pole and the equator.—M. Dupuy read his first memoir on the distillation of the fatty bodies.

July 18.—M. Puissant remarked, by letter, that a portion of MM. Brousseaud's and Nicollet's exposition just mentioned, was founded on his methods and formulæ, and that he made a great part of the calculation.—M. Le Gendre, in the name of a committee, made a favourable report on the analytical memoir by M. Le Jeune.—M. Arago communicated the observations on the declination of 61 *Cygni*, which he had made in conjunction with M. Mathieu. It appears to result from them that the annual parallax of this star,—although it must be regarded as one of the nearest to the earth, from the rapidity of its proper motion,—does not amount to a single second.—M. Arago also explained in the sequel the grounds of his certainty that the telescope he employed in his great work, relative to the diameters of the planets, does not give any sensible irradiation.—M. Cuvier made a favourable report on the zoological collections brought home by the naturalists attached

to

to the expedition under M. Duperrey.—M. Libri read a memoir on the mathematical theory of heat.

July 25.—M. Girard gave a verbal account of a work entitled The statistics of the former department of Montenotte, by the Count Chabrol de Volvic.—M. Chaussier gave an account of the work by M. Huzard jun., entitled On warranty and redhibitory defects in the sale of domestic animals.—From a report by M. Boyer it appeared that M. Cagnon had not attained his object in his memoir relative to the rupture of the rectum.—M. Gambart, director of the observatory of Marseilles, presented the parabolic elements of the comet he discovered on the 19th of May 1825.

LII. *Intelligence and Miscellaneous Articles.*

ASTRONOMICAL INFORMATION.

To the Editor of the Philosophical Magazine and Journal.

Sir,

NEW COMET.

IN the night of the 28th ultimo, a faint appearance of a comet was observed here; but from the lunar light and the passing clouds its true position could not be ascertained. It was again seen here with more favourable weather for observations, between 9 and 10 o'clock P. M. on the 7th instant, nearly in the S.E. point, in the right side of the constellation *Cetus:* the nearest star to it was the one marked υ in the under part of the belly of the Whale. Its train was then inclined to the east, from its position with respect to the sun, and between ten and eleven degrees long, extending to the star Baten in the body of that constellation.

Its nucleus was just perceptible with the naked eye at intervals, and the apparent size of a star of the fourth magnitude, surrounded by a large circular coma fifteen minutes in diameter, and of a brilliant appearance, so that it was seen through attenuated haze. Its distance from the sun was 166 degrees, the first comet we have ever seen at so great a distance from that luminary.

It was again seen here in the nights of the 13th and 14th, with a very low altitude when on the meridian, but not so bright, nor the train so long by four degrees as in the night of the 7th.

Positions.

	Right Ascension.	Declination.	
1825. Oct. 7.	31° 20′	20° 00′	South.
— 13.	15 45	33 30	
— 14.	13 00	35 30	

Its motion through the heavens, in comparison with former observations made on it by other observers, appears to have been very irregular, and accelerated at intervals, varying from one to upwards of three degrees in a day: but it should be recollected that these irregularities and accelerations in its mean motion have been materially effected by the earth's geocentric motion. Its south declination being so great, it was not seen in this latitude after the 16th: but to the inhabitants of southern latitudes this celestial visitor must have become an object of admiration.

In your last Number, pages 217 and 218, the discovery of *five* comets this year by foreign astronomers is mentioned: but from a consideration of the dates of the discoveries, and the positions of these comets, and also the direct south-west motion of the *one* we have just described, we are inclined to think that this one will answer for at least *three* of them; namely, the two seen by M. Pons on the 15th of July and the 9th of August, and the one by M. Harding on the 23rd of August: for it would be an uncommon *phænomenon* indeed, were we to observe in the course of two or three months so many comets in the same hemisphere, and so near together as these are said to have been.

Conjunction of the Planets, &c.—Soon after 4 o'clock in the morning of the 28th ultimo, a conjunction of the planets Mars and Venus and the star Regulus was observed here. These celestial orbs were about 15 degrees above the eastern horizon, in the prime vertical; and the sky being cloudless at the time made this phænomenon very conspicuous. The distance of Venus from Mars was 44 minutes of a degree, and from Regulus 39′ 15″; the three forming an isosceles triangle. Jupiter was 6° 33′ to the eastward of Venus.

An occultation of Regulus behind Venus took place at 6 o'clock in the evening of the same day, and at 10 p. m. Mars and Venus were in their nearest conjunction; namely, two-thirds of a degree distant from each other.

At 4 o'clock in the morning of October the 4th, a conjunction of Venus and Jupiter was also observed here, when the former was 40 minutes distant from the latter. Venus, in this position, certainly appeared the most brilliant of the two, and reflected a greater light upon the earth than Jupiter did. In the evening, Venus, when passing the southern side of Jupiter, was only a few minutes distant from him.

At 5 o'clock in the morning of the 12th of October, the planets Mars and Jupiter were in conjunction, viz. within half a degree of each other; and in the night following, when Mars passed the northern side of Jupiter, they were some minutes

minutes closer together. By the side of Jupiter, Mars did not appear to be larger than a star of the first magnitude.

I am, sir, your obedient servant,

Gosport Observatory, Oct. 20th, 1825. WILLIAM BURNEY.

The following are the particulars of two Comets at present visible in Europe.

The first Comet.

Observatory, Passy, August 21, 1825.

Right Ascension $4^h\ 15'\ 2''{\cdot}96$, at $23^h\ 36'\ 18''$ } Sidereal time
Declination . . $21°\ 40\ 50\ {\cdot}07$ $28\ 43\ 43$ } at Passy.

August 22, 1825.

Right Ascension $4^h\ 14'\ 49''$ }
Declination . . . $21°\ 26\ 0{\cdot}$ ‡ N } at $1^h\ 1'\ 36''$ Sidereal time.

This comet has no visible tail, is very faint, and has the appearance of a nebula. The place on the 21st is tolerably exact; that given on the 22nd is only approximate; it will, however, be amply sufficient to enable observers to find it. It is visible in a night-glass.

The second Comet, or the Comet of short period.

August 21, 1825.

Right Ascension $7^h\ 53'\ 29''{\cdot}31$ }
Declination . . . $28°\ 40\ 24\ {\cdot}45$ N } at $1^h\ 39'$ ‡ Sidereal time.

August 22, 1825.

Right Ascension $8^h\ 1'\ 29''{\cdot}16$ }
Declination . . . $28°\ 9\ 56\ {\cdot}78$ N } at $1^h\ 39'$ ‡ Sidereal time.

This comet also has no appearance of tail; its observed place differs so little from that given in Encke's Ephemeris, that, by placing the instrument according to the data there given, the comet will be easily found. It is not visible in the night-glass, yet it is much more distinct than the preceding comet. The observations here published were made with a seven-feet equatorial instrument, by James South, F.R.S.—*Passy, near Paris*, Aug. 23, 1825.—*Edin. Phil. Journ.*

NORTHERN EXPEDITION.—CAPTAIN PARRY'S RETURN.

We are sorry to announce the failure of Captain Parry's third attempt to effect the North-west passage, which was suddenly frustrated by an unexpected accident, while every thing bore a favourable appearance. It will be seen from the subjoined extract of a letter addressed by a principal officer in the expedition to an eminent scientific gentleman in this city, that the two ships reached Prince Regent's Inlet in September last year. Port Bowen, in which they spent a dreary winter of nearly

nearly ten months, is in lat. 73° and long. 89°. They left that place on the 19th of July, when the ice broke up; and twelve days after this, while working southwards, exploring the inlet, the shipwreck of the Fury compelled them to forgo the further prosecution of their discoveries. It is satisfactory to learn that the ships have not lost a single man. As the progress of the expedition was cut short by an accident, and not by any thing depending on the climate, we think there is nothing in the result to discourage a new attempt. The Hecla was off the mouth of the Frith on Wednesday, and on her way southward. Information as to the general result of the expedition was communicated to the Lord Provost by Captain Knight yesterday; and his lordship, with the most polite attention, immediately gave notice to the newspapers. The extract subjoined, however, supplies various additional details.—*Scotsman.*

"His Majesty's Ship Hecla, Oct. 12, 1825.

"Dear sir,—We sailed from the west coast of Greenland on the 4th of July 1824. In passing Davis's Straits we were beset 58 days in the ice. On the 9th of September we cleared the ice, and on the 13th of the same month entered Barrow's Strait. The winter was now setting in fast; we therefore endeavoured to reach Port Bowen, in Prince Regent's Inlet, which we effected with some difficulty on the 28th. By the 6th of October we were completely surrounded with young ice. The winter passed more agreeably than could have been expected; we had a good library on board, and managed to raise a tolerable masquerade in one of the ships every fortnight. The winter was what might be called a mild one in this part of the world, the thermometer never exceeding 48½ degrees below zero. During its continuance we had fine sport chasing white bears, twelve of which were killed. White grouse were abundant in spring: we shot a great number of them. They were excellent, and proved a great luxury to the officers and men. The summer, which commenced on the 6th of June with a shower of rain, was very fine; the thaw went on rapidly. On the 19th of July the ice broke up, and we bade farewell to Port Bowen, where we had passed nearly ten months. On the 23d we made North Somerset, and worked to the southward along its coast, until the morning of the 1st of August, when unfortunately the Fury was driven on shore by the ice. Every effort was made to save her; but our exertions proving fruitless, she was abandoned on the 19th, and her people taken on-board the Hecla. Thus ended all our hopes of making the North-west passage, which seemed favourable till this accident. On the 1st of September we left Regent's Inlet for England, and made the coast of Scotland on the 10th. We have been extremely

extremely fortunate during the voyage, not having lost a man either by disease or accident."

The following is another account.

The expedition was delayed in the summer of 1824 in getting across Baffin's Bay by a most extraordinary accumulation of ice; and it was not before the 9th of September that they passed that barrier.

On the 26th of September 1824, they arrived at the entrance of Prince Regent's Inlet: after two or three days of bad weather and obstruction from the ice, they got into Port Bowen: and on the 1st of October the ships were safely placed in their positions for the winter. During the spring of 1825, parties of discovery were sent under Captain Hoppner inland to the eastward; under Lieutenant Sherer along the coast to the southward; and under Lieutenant Ross to the northward. Lieutenant Sherer reached Fitzgerald-bay in 72° 20′, and Lieutenant Ross proceeded beyond Cape York, in 73° 30′.

The ships sailed from Port Bowen on the 20th of last July, and on the 22d were driven back again to nearly Prince Leopold's Islands, in Lancaster Sound: on the 24th they got hold of Cape Seppings, on the western entrance of the Regent's Inlet, and they worked down the western shore till the 1st of August, when the Fury was forced on shore by masses of ice; she however was got off, and hove down for repair a little further to the southward; but on the 23d of August they were forced, in consequence of increased severity of weather, to abandon her. Her officers and men were removed into the Hecla, (which had also been in great danger,) and Captain Parry felt himself under the necessity of returning to England, and accordingly stood to the northward, and on the 27th anchored in Niell's Harbour, a little to the southward of Port Bowen. After two or three days spent in refitting, they sailed again on their way homewards, and on the 17th of September got through the ice and passed the arctic circle. On the 10th of October they made the Orkneys.

Only two men were lost during the whole voyage—one drowned, and one by disease. The health, spirits, and discipline of both ships' companies were excellent all through.

Letters have been received at Earl Bathurst's office from Captain Franklin, stating the arrival of the expedition under his orders at Lake Winnipeg, early in June, from whence they intended to proceed to Bear Lake. The persons composing the

the expedition were in perfect health, and the season had been extremely mild and open.

AFRICA.—NIGER.—FERNANDO PO.

In the absence of the details and the results of the late journey of discovery into Northern Central Africa from the proper channel, we are enabled, from a sure source of information, to lay before our readers the following important particulars regarding Clapperton's discovery.—From the information which he obtained, he considers it certain that the mighty Niger terminates in the Atlantic Ocean in the Bights of Benin and Biafra. Sockatoo, the capital of a considerable state, and at which place he turned back, is situated in 12 degrees north latitude, and in about 7 degrees east longitude, and upon a river which flows west by it into the Joliba (the Niger) of Mr. Park, distant about forty miles from the city mentioned. The inhabitants of Sockatoo told Major Clapperton that they traded up the Joliba with Timbuctoo, and down it with the Europeans who frequented the sea-coasts at the mouth of the river. Upon turning to a map of Africa, the reader will perceive that Sockatoo is about four hundred miles from Timbuctoo, three hundred and fifty from the mouth of the Rio de Formosa, very near Bousa, where Park lost his life, and within a short distance of the position, as nearly as can be laid down, of the great city of Wassanah, mentioned to Captain Riley by Sidi Hamet. We have heard of other curious particulars, but these are of minor importance, and sink into insignificance before the great point mentioned and determined. It is with considerable satisfaction we are enabled also to state, that by the activity and attention of the Colonial Office, Major Laing was furnished with an abstract of Major Clapperton's important discoveries, to guide him in his researches and his journey, and that this abstract reached him a few days before he left Tripoli.—He is by this time at or near Timbuctoo, and has taken with him four or five carpenters (Blacks), from the United States of America, in order to assist in building a vessel (of sufficient strength to pass the Rapids in safety) at Timbuctoo, in which he means to descend the Niger to the sea. The period of the year and the state of his health are such as leave the most sanguine hopes of his early and complete success. It is calculated that he might be in the Bight of Benin by the month of March next; but, taking into account the delay and difficulty of travelling in Africa, whether by land or by water, we think this period too early. It is not necessary to unfold a map of Africa, to perceive the commanding position of the island

island of Fernando Po, and to appretiate its value, whether as a station to suppress the slave-trade, or a point and fortress from which to commence and to protect all future operations in Africa, whether these be commercial or political. It is besides a very healthy and fertile spot, and on this account alone ought to claim a decided preference over all our present ill-chosen and sickly stations on the coast of Africa; but when we take into account the commanding geographical position (commanding alike for every purpose) Fernando Po sinks every other place on the coast of Africa into insignificance.—*Glasgow Courier.*

LENGTH OF THE SECONDS PENDULUM IN VARIOUS PARTS OF THE EARTH, AS DETERMINED BY CAPTAIN SABINE.

Stations.	Latitude. °	′	″		Length in a vacuum, reduced to the level of the sea.
St. Thomas	0	24	41	N.	39·02074
Maranham	2	31	43	S.	39·01214
Ascension	7	55	48	S.	39·02410
Sierra Leone	8	29	28	N.	39·01997
Trinidad	10	38	56	N.	39·01884
Bahia	12	59	21	S.	39·02425
Jamaica	17	56	7	N.	39·03510
New York	40	42	43	N.	39·10168
London	51	31	8	N.	39·13929
Drontheim	63	25	54	N.	39·17456
Hammerfest	70	40	5	N.	39·19519
Greenland	74	32	19	N.	39·20335
Spitzbergen	79	49	58	N.	39·21469

Quarterly Journal.

SUGGESTIONS RESPECTING THE FORMATION OF THE PRESENT CRUST OF THE EARTH.

To Richard Taylor, Esq.

Sir,

The pages of your Magazine have been heretofore appropriated to subjects of a geological nature; I presume, therefore, you will not object to the admission of a few lines on that interesting subject, for the purpose of communicating to the public an idea which I have long entertained, and have occasionally mentioned in conversation, and which may possibly serve to explain much of the difficulty which at present remains of accounting for the abundance of marine fossils so much above the level of the present ocean.

I have not leisure, at this time, to enter into particulars, but shall in a few words state the principles of my hypothesis,

leaving for others who have more, to examine and consider the objections that may be urged against them. It has been proved by Sir Humphry Davy and others, that the earths which compose the surface or crust of our globe are not simple elements, but compound substances; for instance,—lime, alumine, silex, &c., are compounded of metallic bases and oxygen: it is also well known to chemists, that the oxides occupy more space than the metals from which they are produced.

If therefore the globe, in its original construction, was composed of elementary substances, these would, particularly near the surface, many of them enter into combination, according to their chemical affinity, and produce the substances we now meet with. This coating would be of considerable thickness, and might for a long period be nearly if not totally covered with water: at the same time there might be a gradual oxidation of the metallic matter, which would produce the different earths in succession, and would be simultaneously precipitated with the remains of marine animals, and lay the foundation for the stratified appearance of what now forms the surface of the earth.

The gradual oxidation of the metallic substance below would naturally absorb and reduce the quantity of water, and at the same time elevate the superincumbent strata already formed, operating in a twofold manner to effect what has appeared so difficult to account for; namely, the great elevation of the remains of marine animals above the surface of the ocean. The force of expansion of the oxides, we can readily conceive, must break and dislocate the former covering, producing all the variety of veins and faults, which would be in after-ages filled up, more or less, by infiltration of metallic and other substances.

I am, yours truly,

Leighton, Oct. 13, 1825. B. Bevan.

MR. FAREY ON HAZEL NUTS FOUND IN PEAT BOGS.

To the Editor of the Philosophical Magazine and Journal.

Sir,

Having observed in p. 225 of your excellent Magazine, an account of the *hazel nuts* lately found at Bonnington, and with a laudable desire for the diffusion of knowledge preserved and sent to you by Sir John Hay, I beg to confirm your account of the state of these nuts*, by mentioning that I have ob-

* The notice here alluded to by Mr. Farey was extracted from the Edinburgh Philosophical Journal, but the reference to that work was accidentally omitted.—Edit.

served

served the same appearances in very numerous instances, I think I may say in every instance, where *a valley bog** has been extensively dug through to its bottom: the nuts, frequently in vast numbers, are all whole, without crack or perforation, and so are the pellicles within the shells, but without remaining vestige of the kernels! These facts suggest two or three interesting subjects of inquiry; viz.

First, Was this country entirely without men or nucivorous animals at the periods when these nuts grew, and fell neglected from the trees or stems?

Second, Did the peculiar insect which now destroys so many of our hazel nuts by feasting on their kernels, and then eating *their way out* through the pellicles and shells (as some naturalists affirm), not exist here when the nuts of our bogs were growing? or if co-existent with these nut-trees, were its eggs deposited in the flowers or embryo kernels, but perished, owing to the wet state of the vegetables amongst which the ripe nuts became deposited? And

Third, Did the nut-trees principally grow *on* the bogs, or were the nuts conveyed by torrents of water into the partial lakes, now often occupied by valley bogs?

I write this in great haste, from the office of my son, Mr. Joseph Farey, which I am daily attending, to transact his business relative to patent inventions, &c., during his unfortunate illness. I am, sir, your obedient servant,

44, Lincolns Inn Fields. JOHN FAREY, Sen.

ON THE FOGS OF THE POLAR SEAS. BY G. HARVEY, ESQ. F.R.S.

It has been commonly supposed, that the fogs which cover the Arctic Seas during the greater part of the summer months are produced by *the moist air depositing its vapour, in consequence of being chilled by contact with the sea.* But this cause, it is presumed, is not adequate to the formation of mists; since it has been proved by Dr. Wells† that dew and hoar frost are the only results which arise from air, either perfectly or imperfectly saturated with moisture, coming in contact with a body colder than itself. To produce mist or fog, as has been

* The distinction here intended, between *valley bogs*, partly composed of silt, and *mountain bogs* of pure peat, occupying high planes of grit-stone chiefly, will be found explained and illustrated by very numerous examples in and near to *Derbyshire*, in my Report on that county, vol. i. p. 308, and vol. ii. p. 347: but wherein, as being a mere abstract, and precursor of my then intended mineral history and large map of Derbyshire, I have omitted to mention the hazel nuts, and many other accompaniments of its boggy tracts.

† Wells on Dew.

satisfactorily demonstrated by Dr. James Hutton*, it is necessary that volumes of air, of unequal degrees of temperature, and holding moisture in solution, should be mingled together; and the circumstances of the Arctic Seas, during the period when these fogs generally prevail, are, it is presumed, in perfect accordance with these conditions.

Before the end of June, the shoals of ice are commonly divided and scattered, the temperature of the ocean being at that time necessarily greater than that of the icy masses floating on its bosom. This inequality of temperature will necessarily impart a corresponding influence to the air, and occasion the portions of the atmosphere resting on the broken surfaces of the water to become warmer than the atmosphere in the vicinity of the icebergs. The cooling influence of the icy masses also, in consequence of their being elevated considerably above the sea, will be diffused, not only by radiations from their upper surfaces to the canopy of the sky above them, but by horizontal radiations to the air surrounding their sides. A volume of the atmosphere therefore, between two neighbouring masses of ice, will necessarily have its middle portion of a higher temperature than that of either of the portions of air between it and the icebergs†; and the consequence of such an unequal distribution of temperature must be, to cause the cold air to mingle with that of a higher temperature, and thus to produce mist or fog. The density of such mist or fog will depend on the difference between the temperatures of the mingling volumes, and on the quantity of vapour contained in the air.

The elevation of those mists above the surface of the sea will also be regulated by that of the icebergs near which they form; since the cooling influence of the frozen mass, by rapidly diminishing above its summit, will as rapidly destroy all tendency in the portion of the atmosphere above the level of the iceberg to assume a condition favourable to the formation of mist; thus prescribing to the mist an elevation dependent on that of the iceberg near which it forms. Captain Ross accordingly remarks, in his account of the Polar Voyage, "that the fog was extremely thick on the surface of the sea, but at the mast-head, and at the top of the iceberg, it was perfectly clear." Captain Scoresby also, in his paper on the fogs of the

* Transactions of the Royal Society of Edinburgh, vol. i.

† If the water in the vicinity of icebergs presents considerable inequalities of temperature, the air which reposes on it must be subject to like variations; and numerous examples of the former are to be met with in the accounts of the polar voyages. Thus Captain Franklin remarks, "the temperature of the surface water was 35° when among the ice, 38° when just clear of it, and 41°·5 at two miles distant."

Polar

Polar Seas, read before the Wernerian Society (Edin. Ph. Journ. vol. vi.) alludes to their definite elevation, and to the sky above them being perfectly clear.

It is possible, however, that two icebergs may be situated so near each other, that their reciprocal horizontal radiations will so cool the volume of air between them as to reduce it to a temperature nearly uniform, and thereby prevent the formation of mist. The cold volume of air so formed, may, however, pass from between the icy masses, and by mingling with the air reposing on the warmer water, beyond the icebergs, produce mist at a distance from them. Nor is it absolutely necessary that *two* icebergs should exist, in order to form mist; since the horizontal radiations of *one*, by cooling the portion of air in contact with it, will cause it to mingle with the warmer air beyond the last-mentioned stratum, and thus create fog. The density of a mist when formed under the latter conditions, will be of a more variable character than when it is formed between adjacent icebergs.

The general diffusion of fogs over the Northern Seas may also be satisfactorily accounted for, from the scattered icebergs separating the water into detached portions; and thereby creating, in innumerable directions, volumes of air possessed of unequal temperatures. The cold air near the icebergs being blended therefore with the warmer air reposing on the middle portions of the broken intervals of water, must form, between most of the floating masses of ice, visible volumes of vapour, having their density dependent on the relative difference of heat between the mingling portions of air, and on the degree of humidity possessed by each.

The cause here referred to, for the production of the Polar fogs, is also one likely to promote their continuance for a considerable time; it being known that the sea continues for many months relatively warmer than the icebergs; and therefore capable, in conjunction with the constant radiation of the ice, of producing that almost constant succession of fogs which cover the Arctic Seas during the greater part of the summer months; and which increase, in so considerable a degree, the difficulties of Polar navigation.—*Quarterly Journal of Science.*

Plymouth, July 19th, 1825.

SELENIUM.

We translate the following from a notice in Schweigger's *Journal* for April last.

The remarkable substance discovered by Berzelius, called *selenium*, is still one of the most scarce and expensive; even the celebrated discoverer complains of the smallness of the quantity

quantity he was able to exhibit. It is true that since its discovery selenium has been found in some ores of sulphur, in the sulphureous mud of some manufactories of sulphuric acid, and in some few kinds of concentrated sulphuric acid itself, yet in such small quantities that the separation of it is still very expensive. 1000 pounds of sulphuric acid containing selenium will scarcely yield a drachm of that substance; and in the largest manufactory scarcely four pounds, in a year, of sulphureous mud will be deposited, yielding, after great labour and expense, at the utmost only from 7 to 8 per cent. of selenium; and from the ores of sulphur themselves it is still more difficult to be obtained. From these causes many chemists have not even seen this substance, much less have they been able to make experiments with it.—Having been so fortunate as to procure a quantity from a source as yet unknown, I offer to sell it to the friends of the science at the moderate price of one Frederic's d'or the drachm, requesting that all orders be sent post-paid, accompanied by the amount; observing at the same time that the present stock [of material] being consumed, I may perhaps not be able to procure a fresh supply.—*Dr. Joh. Barth. Trommsdorf.*

Erfurt, March 1, 1825.

MIGRATION OF BIRDS.

Dr. Schinz, secretary to the Provincial Society of Zurich, has endeavoured to discover the laws according to which the birds of Europe are distributed over our continent. The country in which the bird produces its young is considered as its proper one. The nearer we approach the poles, the more do we find peculiar or stationary birds, and the fewer are the foreign species which make their appearance. Greenland has not a single bird of passage. Iceland has only one, which remains during winter, and leaves it in spring for still more northern countries. Sweden and Norway have already more birds of passage; and we find them increasing in number in proportion as we advance towards the centre of Europe. In the intertropical countries no bird emigrates; to the north they all emigrate. The propagation of birds keeps pace with the quantity of food. Spitzbergen has but a single herbivorous species; for the sea presents more nutriment, and all the rocks and cliffs are inhabited by aquatic birds. In the frigid zone, a much greater number of marsh birds breed than beyond the arctic circle and in the warm countries of Europe. Dr. Schinz also indicates the distribution of the species of domestic fowls; and remarks, that each country has its peculiar varieties of fowls.*—*Bulletin Univers.* (*Edin. Phil. Journ.*)

* For Dr. Jenner's investigation of the circumstances which impel birds to migrate, see Phil. Mag. vol. lxiv. p. 50.

STEAM-

STEAM-BOATS ON THE RHINE.

Coblentz, Sept. 17.—The first experiment in steam navigation has been made on this part of the Rhine, and apparently with success. A steam-vessel belonging to the Netherland Steam-Boat Company, who had previously established steam-boats from Rotterdam to Cologne, left the latter place on Sunday, and proceeded to Coblentz, where the king of Prussia was reviewing the troops of the Grand Duchy of the Lower Rhine. On Wednesday his majesty went on board the steam-boat at half-past eight in the morning, and proceeded down the Rhine in the first vessel of this kind that had descended this part of the river. The steam-boat went at a good rate, and was hailed by the shouts of the people of the numerous villages. It returned here yesterday about ten o'clock, and proceeded towards Mentz, whence it will ascend to Frankfort.

LIST OF NEW PATENTS.

To William Duesbury, of Bousal, Derbyshire, colour-manufacturer, for a mode of preparing a white from the impure native sulphate of barytes.——Dated 29th Sept. 1825.—6 months to enrol specification.

To John Martineau the younger, of the City Road, Middlesex, engineer, and Henry William Smith, of Laurence Pountney Place, London, for improvements, communicated from abroad, in the manufacture of steel.—6th Oct.—6 months.

To Sir George Cayley, of Brompton ,Yorkshire, baronet, for a new locomotive apparatus.—6th Oct.—2 months.

To James S. Broadwood, of Great Pulteney-street, piano-forte maker, for improvements in small or square piano-fortes.—6th Oct.—6 months.

To Thomas Howard, of New Broad-street, London, merchant, for an engine which he intends to call a vapour-engine.—13th Oct.—6 months.

To Nathaniel Kimboll, of New York, but now residing in Falcon-square, London, for a process, communicated from abroad, of converting iron into steel.—13th Oct.—6 months.

To Benjamin Saunders, of Bromsgrove, Worcestershire, button-manufacturer, for improvements in making buttons.—13th Oct.—6 months.

To Thomas Dwyer, of Lower Ridge-street, Dublin, silk-manufacturer, for improvements in the manufacture of buttons.—13th Oct.—6 months.

To Joseph C. Daniell, of Stoke, Wiltshire, clothier, for improvements in machinery for weaving woollen cloth.—13th Oct.—6 months.

To Josiah Easton, of Bradford, Somersetshire, esq., for improvements in locomotive or steam carriages, and also in constructing the roads for the same.—13th Oct.—6 months.

To William Hirst, John Wood, and John Rogerson, of Leeds, for improvements in machinery for raising and dressing cloth.—21st Oct.—6 mon.

To Ralph Stephen Pemmberton and John Morgan, of the parish of Llanelly, Carmarthenshire, for a combined drawing and forcing pump.—21st Oct.—2 months.

To Goldsworthy Gurney, of Argyle-street, Hanover-square, for improvements in the apparatus for raising or generating steam.—21st Oct.—6 mon.

To Lemuel Wellman Wright, of Princes-street, Lambeth, for improvements in the construction of steam-engines.—21st Oct. —6 months.

To Henry Constantine Jennings, of Devonshire-street, Portland-place, Middlesex, for improvements in refining sugar.—22d Oct.—6 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. CARY in London, and Mr. VEALL at Boston.*

Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M.							Clouds.							Height of Barometer, in Inches, &c.		Thermometer. London.			Thermometer. Boston.	Rain.		Weather.		
	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Sept. 26	29·98	61	55·00	88	SW.	...	0·015	1	1	1	...	1	1	1	29·87	29·46	60	66	55	63·5	...	0·01	Fair	Cloudy	SW.
○ 27	30·17	54	.....	70	NW.	0·30	.....	1	...	1	...	1	...	...	30·21	29·67	52	65	54	59	...	...	Fair	Fine	SW.
28	30·32	56	.....	70	NE.	...	.....	1	...	...	...	1	1	...	30·37	30·00	49	63	55	54·5	...	...	Fair	Fine	W.
29	30·24	59	.....	68	E.	...	.....	1	...	...	...	1	...	...	30·20	29·94	56	62	48	60	...	...	Fair	Fine	SE.
30	29·93	58	.....	64	E.	·60	·220	1	1	1	1	1	1	1	29·98	29·63	50	60	60	55·5	...	...	Fair	Fine	SE.
Oct. 1	29·84	62	55·00	83	S.	...	·270	1	1	1	...	1	1	1	29·86	29·50	55	65	60	58	...	·08	Showery	Rain	SE.
2	29·68	64	.....	85	S.	...	·145	1	...	1	...	1	1	1	29·73	29·30	60	64	59	61	...	·18	Showery	Rain	SW.
3	29·61	61	.....	85	S.	·20	·210	1	1	1	...	1	1	1	29·67	29·25	60	65	60	60·5	0·90	...	Showery	Cloudy, rain *p.m.*	SE.
4	29·82	60	.....	81	W.	...	.....	1	1	1	...	1	1	...	30·04	29·35	60	66	55	60·5	...	·09	Fair	Fine	SW.
☾ 5	30·10	62	.....	72	S.	...	·085	1	1	1	...	1	1	1	30·14	29·64	60	66	61	58·5	...	...	Fair	Cloudy [*p.m.*	SW.
6	30·05	65	.....	75	S.	·35	·900	...	...	1	...	...	1	1	30·04	29·54	61	66	60	60	...	...	Fair	Cloudy, rain *a.m.* and	W.
7	29·60	59	55·00	73	SW.	...	·015	1	...	1	...	1	1	1	29·98	29·08	60	60	45	58·5	...	·42	Cloudy	Fine, rain *a.m.* and	S.
8	30·01	64	.....	70	S.	...	·270	1	1	1	...	1	1	1	30·01	29·64	51	63	59	52	...	·03	Fair	Fine [*p.m.*	SW.
9	30·00	54	.....	69	NW.	·25	.....	1	...	1	...	1	1	1	30·05	29·44	57	60	58	55	·95	...	Fair	Fine	SW.
10	30·22	62	.....	78	SW.	...	.....	...	...	1	...	...	1	...	30·35	29·64	58	65	57	63	...	...	Cloudy	Fine	SW.
● 11	30·35	60	.....	74	W.	...	.....	1	...	1	...	1	...	...	30·39	29·87	57	64	54	60	...	...	Fair	Fine	SW.
12	30·14	60	.....	77	SE.	·25	.....	1	...	1	1	1	...	...	30·13	29·72	55	65	57	57·5	...	·07	Fair	Fine, rain *a.m.*	SW.
13	30·18	58	55·05	77	NW.	...	.....	1	1	1	...	1	1	...	30·23	29·66	56	64	56	57·5	...	...	Fair	Rain	SW.
14	30·18	55	.....	92	SW.	...	·040	1	1	1	...	1	1	1	30·18	29·70	50	61	50	48·5	...	...	Fair	Cloudy	W.
15	30·46	48	.....	70	NW.	·40	.....	1	1	1	...	1	...	...	30·55	30·02	46	58	49	49·5	...	...	Fair	Fine	W.
16	30·50	49	.....	75	E.	...	.....	1	...	1	...	1	1	...	30·53	30·05	46	61	48	51·5	...	...	Fair	Cloudy [*p.m.*	SW.
17	30·28	54	.....	74	SW.	...	·010	1	1	1	...	...	1	1	30·19	29·75	47	56	45	49·5	...	·05	Cloudy	Cloudy, rain *a.m.* and	SW.
☽ 18	30·15	48	.....	69	W.	·20	·300	...	...	1	...	...	1	1	29·95	29·73	43	50	54	46	...	·12	Rain	Cloudy, rain *a.m.* and	SW.
19	29·47	53	.....	72	W.	...	·310	...	...	1	...	...	1	1	29·20	28·97	51	52	46	50·5	·65	...	Stormy	Cloud., rain *p.m* [*p.m.*	NW.
20	29·00	41	55·00	69	N.	...	.....	1	...	1	...	1	...	...	29·25	28·80	40	44	35	42·5	...	·97	Fair	Fine, rain *a.m.* and	NW.
21	29·44	41	.....	68	N.	·25	.....	1	...	1	...	1	...	...	29·54	29·12	38	44	40	39·5	...	·03	Fair	Fine [*p.m.*	NW.
22	29·86	46	.....	70	N.	...	.....	1	...	1	...	1	1	...	29·99	29·60	42	50	39	44·5	...	...	Fair	Cloudy	NW.
23	30·10	40	.....	70	NW.	...	.....	...	...	1	...	...	1	1	30·14	29·84	37	45	45	40	...	...	Foggy	Fine	SW.
24	29·98	54	.....	83	NW.	...	·090	1	...	1	...	1	1	1	29·92	29·55	50	56	45	50·5	...	...	Cloudy	Cloudy	SW.
25	29·96	47	55·00	71	NW.	·35	·110	...	...	1	...	1	1	1	29·95	29·53	43	46	37	43·5	...	...	Fair	Fine	W.

THE

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LIII. *On the Variation of Density and Pressure in the interior Parts of the Earth.* By J. Ivory, *Esq. M.A. F.R.S.*

TWO different methods have been employed for obtaining a knowledge of the figure of the earth. One is to measure certain lines upon its surface; the other is to investigate the variations of the force of gravitation as we advance from the equator to the pole. The first of these methods has been pretty extensively carried into execution. We are in possession of measurements made in England, in France, in Italy, in America, in India, at the Cape of Good Hope, at the equator, and in Lapland. The results obtained from so many operations do not agree well with one another, and it is difficult to deduce from them a mean ellipticity in which much confidence can be placed. Nor has the reason of this discrepancy escaped detection. The accuracy of the measurement of terrestrial arcs depends chiefly upon the direction of gravity, which is much more sensibly affected than its absolute force by irregularities on the surface, or by the unequal distribution of density in the interior parts. An inequality on the surface of the earth, producing no sensible change in the total amount of gravity, may nevertheless, by acting laterally upon the plumb-line, cause a considerable variation in estimating the curvature.

We become acquainted with the relative force of gravity at different points of the earth's surface by observing the length of the pendulum that performs its vibrations in a given portion of time. Of late years, this mode of experimenting has been greatly improved, or rather it has been carried to the utmost degree of perfection of which it seems capable. Experiments with the pendulum are also less expensive, and less laborious in the execution, than the measurement of terrestrial arcs. They are well calculated for investigating the mean figure of the earth; since the time of a pendulum's vibration depends upon the absolute force of gravity, and is little affected by minute changes in its direction, which is the cause of so much irregularity in the lengths of a degree of the meridian.

ridian. But there is one thing indispensable to the success of both methods. A degree of the meridian and the length of a pendulum vibrating in a given portion of time, both vary with extreme slowness in advancing from the equator to the pole. A pendulum that vibrates in a second at the equator, requires to be lengthened only about $\frac{2}{10}$ths of an inch in order to oscillate in the same time at the pole. The experiments to be compared must therefore be made at places as distant from one another as possible. Unless this precaution is used, the unavoidable errors of observation will bear a considerable proportion to the minute quantities to be found, and consequently the results will be uncertain and irregular.

Besides the two methods of investigating the figure of the earth by terrestrial experiments, there is another derived from astronomical observation. If the earth and the moon were both spherical, the attractive forces which they exert upon one another would depend only on their quantities of matter and their distance. But as the earth has an oblate figure, the moon acts upon the protuberant matter at the equator, and causes a nutation of the axis, the theory of which is well known. Now if the moon displace the axis of the earth, that luminary must itself be re-acted on, and must undergo some change of position in the heavens. Astronomers have discovered two inequalities of the moon,—one in longitude and one in latitude,—which, being determined by actual observation, lead directly to a determination of the earth's ellipticity by which they are caused.

When the results obtained from all the degrees that have been measured are combined with the pendulum experiments made in England and France, and with the determination derived from astronomical observation, the terrestrial ellipticity has been fixed at $\frac{1}{306}$. But this is only the most probable quantity; for the discrepancies are so great as to prove either some imperfection in the methods employed, or considerable irregularities in the figure of the earth.

Captain Sabine, in the employ of the British government, has, in the course of two voyages, made experiments with the pendulum at thirteen different stations from the equator to Spitzbergen in 80° of north latitude. The distance of the extreme stations is nearly the greatest possible; and this mode of experimenting has, therefore, been carried into execution in circumstances the most advantageous that can be chosen. The ellipticity deduced from these experiments is $\frac{1}{289}$. But the most important consideration attending them is the almost exact agreement of the results obtained by combining them with one another, and with the experiments made in England

and

and France. It is thus proved, that when observations are made at stations sufficiently distant from one another, and are judiciously combined in calculation, the effects of local and accidental discrepancies disappear, and a mean result is obtained demonstrating that the earth is possessed of a figure much more regular than all former inquiries led to expect. The two ellipticities, $\frac{1}{306}$ and $\frac{1}{289}$, are to one another as 15 to 16. It may be impossible to fix with any degree of precision the degree of approximation that may reasonably be expected in this investigation; but it is sufficiently honourable for modern science to have ascertained within such narrow limits a quantity which is known to us only by effects extremely minute and complicated, whether we regard the differences of the degrees of the meridian, or the variation of the length of the pendulum, or the small inequalities of the moon's motion.

It is remarkable that the ellipticity deduced from Captain Sabine's experiments is exactly equal to the proportion of the centrifugal force to gravity at the equator. Supposing the earth to have been originally fluid, the ellipticities of the strata varying in density from the centre to the surface, would be caused by the centrifugal force. If the rotatory motion of the earth, supposing it fluid, were stopped, the ellipticities would disappear, and the figure would change to a perfect sphere. Again, if there were no variation of density from the centre to the surface, the ellipticities of the several strata would likewise be constantly the same. Thus the ellipticities, although caused by the centrifugal force, are yet modified by the densities. If the law of the ellipticities were known, we should likewise know the law of the densities, and the degree of pressure that prevails at any distance from the centre. But in reality our knowledge is confined to the ellipticity at the surface, and to the proportion it bears to the centrifugal force, which is not sufficient to enable us to investigate the density and pressure in the interior parts.

It follows from the relation which is found by observation to subsist between the ellipticity at the surface and the centrifugal force, on the one hand, that the earth, supposing it originally fluid, could not have been homogeneous, or of the same density from the centre to the surface; and, on the other, that the gravitation could not be directed to one point, or centre. For, on one supposition, the ellipticity must be exactly $\frac{5}{4}$ of the centrifugal force, and, on the other, $\frac{1}{2}$ of it. All the experiments concur in proving that the actual ellipticity is considerably removed from either extreme case, although it falls between them; and hence we must conclude that the earth, in a fluid state, could only consist of strata varying in

density from the centre to the surface. The regularity with which gravity is found to vary on the earth's surface would follow from the regular disposition of the fluid strata, and indeed can hardly be otherwise accounted for; and this circumstance affords a strong argument in favour of the supposition of primitive fluidity.

The homogeneity of the earth is extremely improbable. We are acquainted with no fluid that is perfectly incompressible. Every material substance yields in some degree to a compressive force sufficiently great. It is very probable that the increasing densities as we approach the centre of the earth are caused by the weight of the incumbent matter*. But all that we really do know is confined to the external figure of the earth, and to the variation of gravity on its surface. From this knowledge we may be led to form some probable conjectures concerning the density and pressure in the interior parts. In such an inquiry we must be guided by what is generally true of those qualities in the instances that have fallen under our observation. A law of density, however well it agree with the figure of the earth, must be rejected if it lead to an improbable law of pressure. And it would be futile and a mere waste of words to descant upon a law of density and pressure without showing that it is consistent with what is experimentally known of the figure of the earth.

Clairaut first determined the figure of an elliptical spheroid *in equilibrio*, supposing it to be little different from a sphere, to revolve upon its axis, and to consist of strata of variable density. He has given a general equation between the densities and the excentricities. From what has been said, it appears that the variation of density merely modifies the proportion between the excentricities and the centrifugal force, this force being always estimated in parts of the gravitation at the surface of every stratum. It therefore occurred to me that Clairaut's equation might be simplified if, instead of the excentricity, we substitute the proportion it bears to the centrifugal force. Having found my conjecture in some degree verified, I shall now briefly explain the results I have obtained.

Let the radius of the earth's equator be unit, a the equatorial

* "Cette hypothese (que les densités de la terre croissent vers le centre) n'est elle pas fort probable d'ailleurs d'elle même? L'eau est elle le seul fluide que nous connoissions? et ne faut-il pas que les fluides plus pesants, soient plus proches du centre de la terre? le mercure est près de quatorze fois plus pesant que l'eau: la grande compression que souffrent les parties proches du centre de la terre, ne pourroit-elle pas contribuer à rendre la matiere plus compacte et plus dense?"—*Traité sur le Flux et Reflux de la Mer, par* Daniel Bernoulli, cap. iv. § 12.

torial radius, ϱ the density, and e the ellipticity, of any interior stratum; then, π being the circumference when the diameter is unit, the gravitation at the surface of the stratum will be proportional to

$$\frac{4\pi f \varrho a^2 da}{a^2} *.$$

Let g denote the space through which a heavy body falls in a second at the equator, and m the mean density of the earth, that is, the density of a homogeneous sphere equal in bulk to the earth and attracting with the same force; then,

$$g = \frac{4\pi f \varrho a^2 da}{1} = \frac{4\pi m}{3},$$

the integral being taken from $a = 0$ to $a = 1$. Hence the gravitation at the distance a from the centre will be

$$g \times \frac{3 f \varrho a^2 da}{m a^2}.$$

Let ω denote the centrifugal force at the equator, that is, the versed of the arc of the equator that passes over the meridian in a second by the diurnal motion; then $a\omega$ will be the like force at the distance a from the centre; and the proportion of this force to the gravitation at the same distance will be equal to, $\frac{\omega}{g} \times \frac{m a^3}{3 f \varrho a^2 da}$.

Now, put $\Phi = \frac{\omega}{g} = \frac{1}{289}$; and assume,

$$e = x \times \Phi \times \frac{m a^3}{3 f \varrho a^2 da}, \qquad (1)$$

and x will be the proportion of the ellipticity to the centrifugal force, that force being estimated in parts of the gravitation. At the surface we obtain $(e) = (x) \times \Phi$, the symbols (e) and (x) denoting what e and x become when $a = 1$; and, at the centre, we have $e^\circ = x^\circ \times \varphi \times m$, the central density being unit, and e° and x° denoting what e and x become when $a = 0$.

Now, if we observe that the symbol A in Clairaut's equation is equivalent to $\frac{m}{3}$, that equation may be thus written,

$$5 a^2 e f \varrho a^2 da = f \varrho d(a^5 e) + \frac{5 m \varphi}{6} a^5 + a^5 (\mathrm{F} - f \varrho de),$$

all the integrals beginning when $a = 0$, and F being the whole

* No distinction is made here between the gravitation and the attractive force at the equator, nor between the gravitation to a spheroid of small oblateness and a sphere of equal equatorial diameter; because the quantities are to be used in an equation between the ellipticities and the centrifugal force, and the method of Clairaut allows the squares of these small quantities to be rejected.

integral

integral $\int \varrho\, d\, e$ from the centre to the surface. Substitute the assumed value of e in the term on the left side; then

$$5\, m\, \phi\, x\, a^5 = 3 \int \varrho\, d\, (a^5\, e) + \frac{5\, m\, \phi}{2}\, a^5 + 3 a^5\, (\mathrm{F} - \int \varrho\, d\, e);$$

and by taking the fluxions and dividing by $5 a^4\, d\, a$,

$$m\, \Phi\, \frac{d\, .\, x\, a^5}{a^4\, d\, a} = 3\, \varrho\, e + \frac{5\, m\, \phi}{2} + 3\, (\mathrm{F} - \int \varrho\, d\, e).$$

At the surface, $\mathrm{F} - \int \varrho\, d\, e = 0$; and therefore, in that particular case, we obtain

$$\left(\frac{d\, .\, x\, a^5}{a^4\, d\, a}\right) - \frac{3\, (\varrho)}{m}\, (x) = \frac{5}{2}, \qquad (2)$$

the symbols (ϱ), (x), $\left(\frac{d\, .\, x\, a^5}{a^4\, d\, a}\right)$ denoting the particular values at the surface, or when $a = 1$.

Differentiate the foregoing equation, then

$$m\, \Phi\, \frac{d\left(\frac{d\, .\, x\, a^5}{a^4\, d\, a}\right)}{a\, d\, a} = 3\, e\, d\, \varrho;$$

and by substituting the value of e,

$$\frac{d\left(\frac{d\, .\, x\, a^5}{a^4\, d\, a}\right)}{a\, d\, a} = x \times \frac{a^3}{\int \varrho\, a^2\, d\, a} \times \frac{d\, \varrho}{a\, d\, a}.$$

Again, let P denote the pressure at the distance a from the centre; then, the increment of the pressure upon the molecule $d\, \mathrm{M} = \varrho\, d\, a$, being proportional to the force urging $d\, \mathrm{M}$ to the centre, we shall have

$$d\, \mathrm{P} = - \varrho\, d\, a \times \frac{4 \pi \int \varrho\, a^2\, d\, a}{a^2},$$

or,

$$\frac{d\, \mathrm{P}}{\varrho\, d\, \varrho} = - \frac{4 \pi \int \varrho\, a^2\, d\, a}{a^3} \times \frac{a\, d\, a}{d\, \varrho}.$$

If we now introduce a new symbol u, the equations that have been investigated may be thus written,

$$\left.\begin{aligned} \frac{a^3}{\int \varrho\, a^2\, d\, a} \times \frac{d\, \varrho}{a\, d\, a} &= - u \\ \frac{d\left(\frac{d\, .\, x\, a^5}{a^4\, d\, a}\right)}{a\, d\, a} + u\, x &= 0 \\ d\, \mathrm{P} &= \frac{4 \pi\, .\, \varrho\, d\, \varrho}{u} \end{aligned}\right\} \qquad (3)$$

And these equations, which all depend upon the function u, together with the formula (2), contain all the conditions of the problem.

Clairaut applied his equation to determine the progression of ellipticities that would be the consequence of a proposed law of density. He supposed the density proportional to some

some power of the distance from the centre. As the densities decrease from the centre to the surface, the exponent of the power must be negative; and he found that the ellipticities must be proportional to a certain positive power of the distance. Therefore the density would be infinitely great, and the ellipticity evanescent, at the centre; circumstances which render the assumption of Clairaut altogether inapplicable to the case of nature. If we put $\rho = \frac{1}{a^n}$ in the equations (3), we shall find,

$$u = -\frac{n^2 - 3n}{a^2};$$

and this value renders the equation for x *homogeneous*, and conducts immediately to the solution of Clairaut.

In the Memoirs of the Academy of Sciences for 1789, Legendre proposed another law of densities which is equivalent to making u equal to a constant positive number n^2, in the equations (3). In this hypothesis the increment of the pressure is proportional to the increment of the square of the density, which is the law of pressure assumed by Laplace, and which he has shown* to agree both with the figure of the earth as determined by observation, and with the mean density of the matter of which it consists.

If we make $u = n^2$, we obtain from the first of the equations (3),

$$a^2 \frac{d\varrho}{da^2} = -n^2 \int \varrho\, a^2\, da,$$

which may be thus written,

$$a \frac{d(\varrho a)}{da} - (\varrho a) = -n^2 \int (\varrho a)\, a\, da;$$

and, by taking the fluxions,

$$\frac{d\, d(\varrho a)}{da^2} + n^2 (\varrho a) = 0.$$

Now, by integrating this equation on the supposition that the central density is unit, we get

$$\rho = \frac{\sin a n}{a n},$$

which is the law of densities proposed by Legendre. Hence we derive,

$$\int \varrho\, a^2\, da = \frac{1}{n^3} (\sin a n - a n \cos a n),$$

$$m = \frac{3}{n^3} (\sin n - n \cos n),$$

$$(\varrho) = \frac{\sin n}{n}.$$

* *Mécanique Céleste*, livre 11^me^. cap. ii, § 6.

Again,

Again, excluding from the value of x quantities that become infinite when $a = 0$, we get from the second of the equations (3),

$$x a^5 = \frac{C}{n^2}(3\int \rho a^2 da - a^3 \rho),$$

$$\frac{d \cdot x a^5}{a^4 da} = \frac{C}{a^3} \cdot \int \rho a^2 da :$$

and at the surface when $a = 1$,

$$(x) = \frac{C}{n^2}\left(m - (\rho)\right),$$

$$\left(\frac{d \cdot x a^5}{a^4 da}\right) = C \times \frac{m}{3}.$$

If these last values be substituted in the formula (2), C will be found, and n will be the only quantity left indeterminate. The calculation being made, I have found these definitive formulæ which agree with the determinations of Legendre and Laplace, viz.

$$\lambda = \frac{n^2 \tan n}{\tan n - n}$$

$$\frac{(\varrho)}{m} = \frac{\lambda}{3}$$

$$(e) = (x) \times \varphi = \varphi \times \frac{5(3-\lambda)}{2(n^2 - 3\lambda + \lambda^2)}.$$

Now, if we take n equal to an arc of 138°, then,

$$\lambda = 1{\cdot}578$$

$$\frac{(\varrho)}{m} = 0{\cdot}526$$

$$(e) = \varphi = \frac{1}{289}.$$

The ellipticity agrees with the experiments of Captain Sabine. If we suppose the mean specific gravity of the earth to be 5·48, that of the outer crust will be 2·88, which is about the specific gravity of the rocks that compose the mountain Schehallien, and therefore not far from the truth.

With regard to the pressure, we have, from the last of the equations (3),

$$\frac{d P}{d \varrho} = \frac{4\pi}{n^2} \times \rho = \frac{4\pi}{n^4} \times \frac{\sin a n}{a n}.$$

It thus appears that $\frac{d P}{d \varrho}$ increases from the surface to the centre; that is, the increment of pressure requisite to produce a given increment of density is a quantity continually increasing in descending to the centre. Now this is a property that may be fairly, if not necessarily, ascribed to the law of pressure that actually prevails in nature with regard to fluid and solid bodies. The greater the compression, the greater a resistance

sistance, it is reasonable to suppose, will be made to further compression. But in one respect the hypothetical law seems to be at variance with experiment. In all small pressures it is found that the increments of pressure and density are nearly proportional to one another. We should therefore expect that the quantity $\frac{d\,\mathrm{P}}{d\,\varrho}$ must be nearly constant at the earth's surface, then increase slowly, and afterwards more rapidly near the centre. But the assumed formula is just the opposite of all this. For it varies most rapidly near the surface, while the arc $a\,n$ decreases to 90°; it then increases more slowly, and near the centre it is almost constant.

It may likewise be observed that the solution is not commensurate with the nature of the problem. For there are two phænomena to be deduced from the law of pressure; namely, the ellipticity, and the proportion of the superficial, to the mean, density. These quantities are no doubt connected; but as their relation must vary with the law of pressure, the definitive formulæ, instead of one indeterminate quantity, should contain two, in order to represent the values of the ellipticity and the superficial density determined by observation.

But it was never intended by the illustrious author who has treated of this problem, to substitute a hypothetical assumption in the place of a law of nature. The particular law of densities was adopted by Legendre, and the law of pressure by Laplace, because in both cases an exact solution was obtained by means of formulæ of easy calculation. This is an advantage not to be altogether disregarded in cases where, from a want of knowledge, we are reduced to make hypotheses the foundation of our reasoning. From the calculations that have been made no inference can be drawn with regard to the ellipticity or the superficial density; but we may conclude legitimately, that the compression produced by the weight of the incumbent matter is sufficient to account for the increasing density in the interior parts of the earth, and for the deviation of its actual figure from that which it would possess on the supposition of a homogeneous fluid assumed by Newton.

Oct. 5, 1825. JAMES IVORY.

LIV. *On the unequal Evolution of Heat in the Prismatic Spectrum.* *By Dr.* T. J. Seebeck *.

LANDRIANI†, one of the first who made experiments on the heating power of the coloured light given by the prism, found the greatest heat in the yellow rays. But Rochon‡ places the maximum of heat between the yellow and the red, in a point which he calls sometimes *orangé*, sometimes *jaune orangé*. Sénebier§ gives as the result of many experiments, that the red light was always warmer than the violet, and the yellow sometimes warmer than the red; and in the examples given by him as a confirmation, and which he says are the mean of many experiments, the yellow light is found to be the warmest. Herschel's experiments‖ differ from all the rest. He found the strongest heat neither in the yellow

* From Schweigger's *Journal*, Band x. p. 129. This paper was read before the Royal Academy of Sciences of Berlin, March 13, 1819, but has only recently appeared in their Transactions. The German editor gives the following statement of his reasons for reprinting it:

"Although it may be supposed that extracts from the memoirs of literary societies, even those of Germany, may be desirable for persons who have no access to larger libraries, I have nevertheless been very sparing of them, unless the authors themselves have had the kindness either to send me copies of their productions, or, what is always most desirable, have made such extracts themselves. In the present instance it is intended to give merely the results of the experiments, and to mention the experiments themselves only so far as may seem necessary to understand their nature. The special cause, however, for publishing this extract now, is an announcement contained in the Annals of Philosophy for Nov. 1823, p. 394, relative to a work on this subject intended to be published by Mr. Powell; and from which it would seem that Mr. P. has overlooked several essential points, which the reader will find in Seebeck's memoir. We will, however, wait for Mr. P.'s publication, and extract from it whatever may be required in addition to the present."

The series of papers on the subject by Mr. B. Powell will be found in the Annals of Philosophy, N. S. vols. vii. viii. and ix.; and one in the Philosophical Transactions for 1825, part i., or Phil. Mag. vol. lxv. p. 437. An extract from Dr. Seebeck's paper, being the only notice of it which has yet appeared in this country, was given in the Annals of Philosophy for September 1824, and commented upon by Mr. Powell in a subsequent number of the same work.

† S. Volta, *Lettere sull' Aria infiammabile nativa delle paludi.* Milano 1777, p. 136. The experiments of Landriani are said to be contained in the *Scelta d'Opuscoli interessanti*, vol. xiii., which I have not been able to procure.

‡ *Recueil des Mémoires sur la Mécanique et la Physique*, p. 348—355. Paris 1783. Rochon's experiments were made during the summer months of 1776.

§ *Phisikalisch-chemische Abhandlungen über den Einfluss des Sonnenlichtes auf alle drei Reiche der Natur.* vol. ii. p. 37. Leipz. 1785.

‖ Phil. Trans. for 1800, p. 255—326, and p. 437—538.

nor

nor in the red rays, and indeed not in the colours at all; and it is still well remembered what a sensation was created by this discovery, as well as by his doctrine of invisible rays deduced from it, especially of heating rays emanating from the sun; what doubts and contradictions it experienced on one side, and what approbation it met with on the other. It is also known that Leslie*, one of the first antagonists of Herschel†, says that he found not the least change on his photometer, which surpasses in sensibility the best of thermometers, either above or below the spectrum, but that the greatest heat is found within it, and only in the red rays; that Englefield‡, on the other hand, soon after confirmed Herschel's discovery, and affirmed that the greatest heat always fell beyond the red.

Seeing results so contradictory, from the experiments of such celebrated natural philosophers, it was obvious that only the difference of apparatus, or of the processes employed in the investigation, could have occasioned them; and that (as is but too probable in a new field of inquiry) things have been overlooked, or not sufficiently noticed, which must have influenced the results. This consideration alone called for inquiry: but I was still more imperatively urged to it from the desire of gaining ocular information on the effect of coloured illumination in all the functions of light, and to inquire whether the *polar contrast* of colours discovered by Goethe would also become proved and confirmed in the effect of coloured light on bodies. I first engaged in an investigation of its chemical effect and its effect on muriate of silver, some of the most important of which I communicated in Goethe's *Farbenlehre* (System of Colours). In the year 1806 I undertook a series of experiments on the excitation of heat in the prismatic spectrum, and continued them in the summers of 1807 and 1808. I shall give a detailed account of their results.

For the first experiments I made use of a mercurial thermometer with a blackened bulb. There were distinct differences of temperature between the blue and violet half, as compared with the red and yellow half of the spectrum; they were however but small, so long as the coloured light fell immediately from the prism on the bulb of the thermometer; yet the difference of temperature of the individual colours on

* If any one be desirous to know more of the photometer employed by Leslie, we would recommend the perusal of a tract lately published, entitled A short Account of Experiments and Instruments depending on the Relation of Heat and Air to Moisture; by J. Leslie.

† Nicholson's Phil. Journ. vol. iv. p. 344.

‡ Journ. of the Roy. Inst. 1802, p. 202.

the same side could not for the most part be noted. The effect might, indeed, be raised by means of lenses; but I seldom made use of them, considering that the more an apparatus is complicated, the more difficult it is to arrive at a certain result by its means, and that they could only be properly applied with a heliostat, in which all the parts of the apparatus can be securely fixed, but which I did not possess. Air-thermometers seemed upon the whole to deserve the preference, since their great sensibility led me to expect that they would give decided indications even immediately in the prismatic light, and for small differences of temperature. I first intended to employ one of Leslie's photometers; yet as the one I ordered was not delivered at the right time, I determined to try whether, and to what extent, an air-thermometer with a simple bulb might be used in these experiments. The result surpassed my expectation, and I therefore preserved it in all my subsequent trials. There were certainly some difficulties in the use of this instrument, but they were diminished by more practice. One difficulty, however, I could not obviate; viz. the variations in the thermometer, produced by the formation of clouds, or even any slight vapours rising before the sun (a thing which frequently occurs even when the atmosphere is apparently clear). There was no other remedy against it except frequent repetition of the experiments with the same prisms in the clearest days,—those most free from vapours; and this I employed accordingly. The best days for such experiments were those after a thunder-storm, or in clear weather after rain; when the results accorded best. From a great number of experiments, I shall here give only those which are marked in my journal as having been successful,—such as in which neither the height of the barometer, nor of the mercurial thermometer in the place where the experiments were being made, underwent any material alteration, at least during the time of one particular experiment.

The apparatus just mentioned, consisted of a thermometer tube 15 inches long, with a very thin bulb of half a Paris inch in diameter. The bulb was uniformly stained with China ink; and a scale made of thin pasteboard, divided into Paris inches and lines, was fixed against the tube. This scale was removed one inch from the bulb, and had there its zero. Thus the bulb and one inch of the tube were quite free. Within the tube was a drop of a coloured liquid, which, after the whole length of the tube had been properly moistened, filled about one inch. The instrument, thus prepared, was placed on a stand which could be raised and lowered at pleasure, and moved, sometimes in a horizontal sometimes in a vertical position,

sition, into the individual colours of the prism, and kept in each until the liquid became stationary. The light under examination only fell on the bulb. A moveable screen, fixed at a distance of from 12 to 18 inches from the thermoscope, intercepted the remaining prismatic colours. The screen was so made that the opening through which the light fell could be enlarged or contracted *ad libitum*. Sometimes only half the screen was employed, at others it was removed altogether; but this is mentioned every time it was done.

The room in which the experiments were made (mostly between 11 and 12 o'clock) lay facing the south, was large, and could be completely darkened. The prism was sometimes fastened in the window-shutter, and no other light was admitted into the room but that which passed through it. But in most cases I let the light fall through an opening in the shutter of about 49 square inches on the prism, placed on a moveable stand on the window-board. The refracting angle of the prism was always directed downwards, its whole side surface being left free, and only the upper third surface covered with black paper; which was also done sometimes with the lower angle, in order to obtain the coloured image more strictly confined, and to keep off all improperly reflected light. The prism had at the beginning of the experiments such a position, that the two angles in which the light entered and passed out were equal, which position I will call, for the sake of brevity, the *normal* position. In order to throw the colours on the thermoscope, the prism was afterwards sometimes brought into a different position by being turned on its axis; but for the most part I left the prism unchanged, and brought the bulb of the thermoscope, by raising or lowering it, into the different prismatic colours. That this latter process could be adopted without prejudice, (at least in cases when the thermoscope was carried down from the cold into the warmer colours,) I was convinced by experiments made on the temperature of the room at different heights, which showed that the air towards the floor (which consisted of clay) was always a little colder. If therefore the warmth increased whilst the thermoscope was lowered into the prismatic spectrum, the experiment was thereby more strongly confirmed. By way of greater security, however, a second air-thermometer was sometimes placed by the side of the spectrum, of which instances will be mentioned.

The glass prisms whose effect was tried were of different qualities and sizes, varying from three-fourths of an inch to one inch and a quarter in breadth of surface. They also differed in the refracting angle, although not considerably, as it

approached

approached in most of them to an angle of 60°. With some of those that deviated more, experiments were made with the largest and smallest angle, without any material difference of effect resulting from them.

As I shall make use in the sequel of the expressions "beyond the limit of the red, or the violet," beyond the limit of colour generally, I will explain here once for all, that in every experiment mentioned I have retained the limits of colours adopted by Newton and most other natural philosophers; and thus, like Herschel and other observers, placed the limit of the red where the bright prismatic red ceases, losing itself in a smaller edge of a fainter red; and that I have assumed the whole of the coloured spectrum as being included within the boundary wherein the colours present themselves to the naked eye.

I cannot indeed confine the prismatic spectrum within these bounds, as will be more fully shown below; yet I did not wish to deviate here, as I did not in my former treatise, from the general manner of expression, because such a practice would have rendered a comparison of my observations with those of others more difficult.

Although I think I may trust my eyes in recognizing and distinguishing colours, I have nevertheless, for the sake of greater security, engaged other persons with eyes sensible to colours, to determine the limits of the spectrum, and found their determinations to agree with mine.

I have yet to observe, that the thermometer was emptied every time after being used, and filled afresh for every new series of experiments; whence the inequality in the figures in the experiments made at different times, which however agree in all other points.

These experiments are as follows:

Exp. 1.—A prism of white Bohemian glass (No. 1). Distance of the thermometer from the prism six Paris feet. The room quite dark, and a screen intercepts the prismatic colours, which are not to fall on the bulb. The spectrum very vivid and well limited. The thermoscope stands

	″	‴
One inch above the violet at	1	$2\frac{1}{4}$
Half an inch ditto	1	$2\frac{1}{4}$
Rises, on the screen being lowered, to	1	$2\frac{1}{2}$
In the violet	1	3
—— blue	1	4
—— a little more light falling on the tube of the thermoscope	1	$6\frac{1}{2}$
Half in the green	2	2

Deeper

	″	‴
Deeper in the green	3	0
Ditto without screen	3	8
In the yellow—ditto	4	9
In the red	5	11
Close under the red (yet a reddish glimmer fell on the upper part of the bulb)	5	5
Half an inch below the red	4	0

I must observe, that the measurements here given are all from the highest point of the bulb to the limit of the colour. If the distance of the centre of the bulb be required, 1-4th of an inch must be added to the given distance.

Thus in this prism the greatest heat showed itself in the red; and the difference between that and the violet was 4″ 8‴.

Exp. 2.—With the same prism, No. 1, on another day. Distance of the thermometer six feet.

Height of the thermometer by the side of the coloured spectrum at the elevation of

	″	‴
Half an inch above the violet	5	$0\frac{3}{4}$
Half ditto above the prismatic violet itself	5	$2\frac{1}{2}$
Quarter ditto	5	$3\frac{3}{4}$
Close above the violet	5	5
Half in ditto	5	8
In full violet	6	$5\frac{1}{2}$
Half in the blue	6	$9\frac{1}{2}$
In blue	7	5
Half in greenish yellow	8	3
In the yellow	9	$1\frac{1}{2}$
Half in the red	9	9
Entirely in the red	10	7
Half in the red, half below it	10	3
Close under the red	10	1
also to	10	3
Quarter of an inch below the red	9	3
Half ditto ditto	8	0
One ditto ditto	7	0
and	6	9
Two ditto ditto	6	1
At an equal height without the spectrum	5	6

In this experiment then, an increase of temperature was shown even half an inch above the violet; and the difference between the heat in the shade and that in the light, half an inch above the violet, amounted to $1\frac{3}{4}$ lines. The difference between half an inch above the violet and the maximum of heat, which again fell in the full red, 5 in. $4\frac{1}{2}$ lines. But it must be noticed

noticed that the temperature of the room had increased by $5\frac{1}{4}$ lines during the experiment between the two extremes.

Exp. 3.—With the same prism, No. 1. The thermoscope six feet distant. Only made concerning the excitation of heat in and below the red. The second thermoscope stands near the spectrum at the distance of four inches.

	1st Thermoscope, in the Spectrum.	2d Thermoscope, near it.
Station at the beginning of the experiments	7″ 0‴	5″ 4‴
Four inches below the red	7 $0\frac{1}{2}$	5 4
Three inches and a half ditto	7 $1\frac{3}{4}$	
Three inches ditto	7 $2\frac{1}{2}$	5 4
Two inches and a half ditto	7 $3\frac{1}{2}$	
Two inches ditto	7 $6\frac{1}{2}$	5 $3\frac{1}{2}$
One inch and a half ditto	8 $1\frac{3}{4}$	
One inch ditto	8 $10\frac{1}{2}$	5 $3\frac{3}{4}$
Half an inch ditto	10 $8\frac{1}{4}$	
Close under the red	13 9	5 $3\frac{3}{4}$
In the red	14 3	5 $3\frac{3}{4}$

This experiment, which was made under the most favourable circumstances on the 19th of September 1807, may be considered as being decisive; and it not only serves to confirm the results of the two preceding ones, but also shows that the effect on the thermoscope extends, within the distance mentioned, to four inches beyond the limits of the red, and the entire spectrum.

It will be useless to mention a greater number of experiments concerning the differences of temperature of the yellow and red half of the rays from this prism; I will only observe, that all which were made under favourable circumstances gave the same result.

I shall now give a few experiments on the elevation of temperature beyond the blue and violet half, of which the second experiment has already given us an instance.

Exp. 4.—The prism, No. 1, at the same distance from the thermoscope.

Station of the thermometer by the side of the spectrum one inch and a half above the violet	4″ 10‴
One inch and a half above the violet itself, remained	4 10
Half an inch above the violet	4 $10\frac{1}{2}$
Close above ditto	5 0
Half in ditto	5 $1\frac{1}{2}$
A little deeper in ditto	5 $5\frac{1}{2}$
In full ditto	6 0
Outside the spectrum	5 $0\frac{3}{4}$

Exp. 5

Exp. 5.—Same prism; distance from thermoscope four feet.

One inch and a half above the violet, by the side of the spectrum	5″	$5\frac{1}{2}'''$
One inch and an half ditto, in the spectrum . .	5	$5\frac{1}{2}$
One ditto, ditto	5	$5\frac{3}{4}$
Half ditto, remains	5	$5\frac{3}{4}$
In the violet	6	2
In ditto, and a little blue	6	9

Thus again, in both experiments, an elevation of temperature beyond the violet. However, it ought to be mentioned that in frequent instances the thermoscope here showed no increase of heat.

Exp. 6. (22d September 1806.)—Another prism, No. 2; distance of thermoscope from it two feet two inches.

The prism exhibited the greatest heat outside of the spectrum, particularly at a quarter of an inch below the red *. The difference between the station here and in the violet amounted to four inches two lines. The spectrum itself was very brilliant, the prism being one of the clearest and purest that I ever used. This instrument, together with another of similar size and perfection, is kept in the Duke's cabinet of natural philosophy at Jena. The second prism was also found similar in effect to the first. The specific gravity of these two prisms is, according to M. Voigt of Jena, 3·2482, at 61°·25 F. They are therefore of flint-glass, and, as I suspect, of English manufacture. I must add that the two prisms are equilateral, the breadth of each side being about one inch.

I made with the prism No. 2, two other experiments (7th and 8th); and all three as perfectly accorded with each other as could be expected from experiments of this kind. The difference between the violet and the red, in the 6th experiment, was four inches two lines; in the 7th, four inches one line; in the 8th, four inches. The deviations in the difference between the red and a quarter of an inch below it were greater; for in the first instance thirteen lines; then, twice, nine lines; and in the last case, eight lines. The mean of this would be nine lines and three quarters. That this shows the difference between the heat in the red, and a quarter of an inch below it, rather below than above the truth, may be seen from the following experiments.

Exp. 9.—Distance from thermoscope seven feet, as in experiments 7 and 8, and with a very clear sky.

* The experiments were conducted like the former. It will, therefore, be sufficient here and henceforward only to mention the results.—German Editor.

In the red	6″	9‴
One inch below the red	7	0
Three lines	7	10

Difference thirteen lines.

Exp. 10.—On another day. The distance the same.

In the red (without screen)	7″	9‴
Quarter of an inch below the red	8	5

Difference eight lines.

Exp. 11.—In the red . . . . . 7 $9\frac{1}{2}$

Long remained so.

Quarter of an inch under the red, it rose in a few seconds to	8	3
And at last (where it remained stationary) to	8	10

Difference thirteen lines and a half.

Exp. 12.—In the red 7 1

Below it 7 11

Difference ten lines.

The mean of all these experiments would amount to ten lines three-eighths.

Exp. 13.—For the purpose of comparing the effect of the flint-glass prisms Nos. 2 and 3, during a perfectly clear sky, 30th May 1808.

The difference between the station of the thermoscope in the place of experiment, and the maximum of heat immediately below the red, amounted to four inches two lines; the difference between the heat in the red and the superior heat close under the red, twice, seven lines; and three times, six lines. Both prisms produced absolutely the same effects.

Some experiments were now made with the same prisms, 2 and 3, at a distance of four feet (Experiments 14—18); and the result was, that the mean difference between the heat in the red and the superior heat beyond the red, for the distance of the thermoscope of four feet, amounted to fourteen lines.

All other trials made with these prisms gave the same result, the maximum of heat being always found beyond the limits of the spectrum.

I now refer to experiments made with other prisms (Nos. 1, 5, and 6) of Bohemian glass; and first a series of them made on the favourable 30th May 1808, immediately after the 13th experiment mentioned above. (Experiments 19—23.)

These experiments, compared with the 13th, gave the most convincing proof that glasses of different composition and qualities also differ in their effects. For in all these experiments (from 19 to 22) made with prisms approaching to crown-glass in their refraction

refraction and dispersion of colours, the maximum of heat fell within the red, whilst in flint-glass it always fell below it. The prism No. 6 seems to have the maximum of heat on the boundary of the red.

I cannot omit mentioning here a result of another kind, which goes to confirm the different effect of the species of glass just named. White muriate of silver, as I have detailed elsewhere *, is changed in the spectra given by prisms of common white glass (among the rest in that of the prism No. 1) in the following manner. In the violet it turns to a reddish-brown colour, and even beyond the boundaries of that ray; in the blue it turns blue, or blueish-gray; in the yellow it remains nearly unchanged, or at most assumes a slight yellowish tinge; but in the red, and mostly also a little beyond it, it turns red itself. In some prisms (I have also said) this redness fell entirely without the red of the spectrum,—they were such with which the greatest warmth was produced beyond the red; and I may add here, that it was with the prisms Nos. 2 and 3 that I made this experiment.

I undertook some further experiments with the prism No. 6 (Experiments 24—28); and another prism, No. 7, in Experiment 27, was pretty equal in effect with prism No.6; the highest point of heat appearing also with this close to the limit of the red, and perhaps a little beyond it. An eighth prism, of the colour of the smoky topaz, proved exactly the same; the greatest warmth being for the most part on the limit of the red, although the temperature was sometimes found equal in the red itself and immediately below it. The effect of another prism, No. 9, of a yellowish tinge, with which the Experiments 29 and 30 were made, approached that of the three former. Its considerable weight and strong refraction and dispersion of colours make it probable that it contains lead, which may also have given it its yellow colour. A prism of German flint-glass, on the other hand, gave exactly the same result (Experiment 31) as those of the prisms Nos. 2 and 3 of English flint-glass; the greatest heat was observed when the bulb of the thermoscope stood three lines below the limit of the red.—But three other prisms (No. 10 of common glass, No. 11 of white Bohemian, and No. 12 of white glass) had the same results (in Experiments 32—34) as Nos. 1, 4, and 5: they showed the greatest heat when the bulb of the thermometer stood in full red.

I made also several experiments with liquids, of which I shall select a few.

* Goethe's *Farbenlehre*, vol. ii. p. 718.

Exp. 35. (7th September 1806.) A prism filled with water, whose refracting angle was about 40°. The panes of glass which inclosed the water had a breadth of about four inches. The prism was brought as near as possible to the normal position, and the spectrum received at the distance of about five feet. The same water-prism was applied in Experiment 36. The thermometer at the distance of four feet. In both experiments the colourless light in the centre of the spectrum always possessed the greatest degree of heat; the next in degree was the light above the yellow. In the red, however, the temperature was much lower; the difference amounting to an inch. It is also remarkable, that the temperature in the white border under the blue approached that in the red, it being but three lines higher. I must not omit to state, that a second and weaker yellow and red (as first observed by Ritter in glass prisms) appeared also in this prism, immediately below the blue.

Two other experiments (37 and 38) were made with this water-prism, at distances from the thermometer of from four to five feet. Both experiments evinced that close under the red the heat is much less than in the red or in the yellow. And Experiment 39, made at the distance of six feet, not only confirmed the preceding experiments, but proved also that the heat in the yellow exceeds that in the red*, and this at the distance mentioned, (yet all other conditions being balanced,) by one inch.

Exp. 40.—The prism was filled with a solution of sal ammoniac and corrosive sublimate, a mixture to which Blair, in his treatise on aplanatic telescopes†, ascribes a particular power of dispersing colours, and whose application therefore, for the purpose of a comparison with the effect of water, might be found useful. This solution was quite colourless and transparent, the dispersion of colour very considerable, and the great extent of the red colour particularly striking. Distance of the thermoscope from the prism four feet and a half.

The highest temperature in this case was also in the white, and below the red a less degree of heat than in the red; and according to two other similar experiments (41 and 42) the place of the greatest heat with this refracting medium seems to be between the yellow and the red, but nearer the former than the latter. Even when the equilateral prism used in Experiment 42 was filled with colourless concentrated sul-

* The same result was obtained previously by Wünsch. See *Mag. d. Gesellschaft naturforschender Freunde in Berlin*, 1807, 1 *Jahrg.* 4 *heft.* He also found the greatest heat in the yellow with prisms filled with spirits of wine and oil of turpentine.

† *Gilb. An. d. Phys.* vol. vi. p. 129.

phuric

phuric acid, the maximum of heat seemed to fall between the yellow and the red. I must however observe, that the experiments with the two liquids last named belonged to those which were made last, and were therefore not so frequently repeated as might have been necessary, not having been made under the most favourable circumstances. With sulphuric acid this was the only experiment.

In the preceding experiments the prisms had generally the normal position; and their refracting angles being always turned downwards, the spectra had, at least at the beginning of every series of experiments, the lowest station.

But as (as has been observed) the prismatic colours were sometimes carried over the thermoscope by turning the prism round on its axis, and therefore as considerable deviations from the above position occasionally took place, it was necessary to know what influence this circumstance might have had on the results, and in fact whether the effect on the thermoscope was varied by opposite positions, viz. with the greatest and smallest angle of incidence of the light; by which, in both cases, the spectrum would be placed higher, with this difference, that in the former case it would be more compressed, and in the latter more enlarged, than in the normal position.

For this purpose I made comparative experiments with prisms of flint and crown glass (Experiments 44 and 45). I observed that the heat in the spectrum decreases when the incident angle is greater or smaller than it is in the normal position; but the place of the greatest heat is not changed, falling in the flint-glass always beyond the limit of the red. The same experiments were made with prisms of Bohemian glass (Nos. 1 and 4), and with a greater angle of incidence, when the proportion remained the same as in the normal position, the heat in the red being greater than below the red, but the difference being much less: whilst with smaller angles it was the same in both, and one inch below the red, greater than in the green. The experiments therefore with these two prisms, resembling crown-glass, also confirm the above axiom,—that the heat is always greatest in the same prismatic colours in the normal position, and is reduced in proportion to the distance of the prism from that position. But then the brilliancy of the colours and the intensity of the light in general is also much diminished, which ought well to be borne in mind. The experiments also confirm the assertion, that the difference of warmth of those parts of the spectrum which closely border upon each other decreases, the more the position of the prism deviates from the normal, which is a result of the greater dispersion of light and the decrease of its intensity.

By

By the results of these last experiments those of the former were not only confirmed, but obtained additional weight; for it appears from them, that the differences have been found rather too small than too great; since the elevation of the spectrum above the normal station, whether effected by the turning of the prism or the change of position of the sun, could not but have an injurious effect on the results obtained.

Thus the differences of temperature in the different prismatic colours will be most distinct with the normal position of the prisms, in clear days without exception. But it is different when the atmosphere is impregnated with vapours, especially when they begin to collect before the sun. For then I found repeatedly, even in this favourable position of the apparatus, but very small differences in the heat of colours closely bordering on one another, especially also between those of the red and yellow half of the spectrum.

The light of the sun then assumes a yellowish tinge. It was to this circumstance that I thought the deviating effect should be chiefly attributed. This occasioned the following experiments:

Exp. 52.—A prism of white Bohemian glass, which in clear days always gave the greatest heat in the red (viz. No. 4 of the above), in the normal position, at a distance of six feet from the thermoscope. Its station in the place of experiment being $5''\ 11'''$.

a. An orange-coloured glass fixed to the inner surface of the prism.

In full red	$8''\ 8\frac{1}{2}'''$
Close under the red	8 10

b. The same glass fixed against the outside face of the prism; an hour later.

In the red	$8''\ 11'''$
Immediately below the red	9 0

Exp. 53. (On another day.) The same prism, apparatus, and distance. Station of the thermoscope in the room where the experiment was made, $5''$. The orange-coloured glass outside the prism.

a. In the red	$6''\ 7'''$
Close under the red	7 2
b. In the red	6 7
Close under the red	7 2

The heat under the red was thus always greater, but the difference very unequal.

c. Without a coloured glass.

In the red	$10''\ 4'''$
Close under the red	10 4

These

These experiments have not been carried further, as would have been desirable, since the last experiment was not made on a favourable day. Their result, however, seems to be, that the effect of the prismatic red was more weakened by the orange-coloured glass, than the glimmering light beyond it, which had a yellowish tinge proceeding from the glass. For that the effect of the prismatic light is weakened generally and more considerably by the coloured glass, appears by the last experiments.

On the same day, too, experiments were made with the flint-glass prism, No. 2, with the intention of inquiring whether the opposite colours (which, if they do not destroy, at least weaken, the prismatic colours) would produce a considerable effect.

Exp. 54.—Distance of the thermoscope, six feet; station of the same in the place of experiment, $5^{II}\ 1^{III}$.

a. A violet pane of glass fixed to the outer side of the flint-glass prism.

In the red	5^{II}	4^{III}
Close under the red	6	2

b. The violet glass on the inner face of the prism; an hour later.

In the red	6^{II}	0^{III}
Close under the red	6	9

The difference in the first experiment was 10^{III}, and in the second 9^{III}; and in both instances the heat was, as usual in this prism, greater under the red than within it.

Exp. 55.—The same prism, with a blue glass fixed to its outer surface; the distance of the thermoscope as before.

In the red	7^{II}	2^{III}
Close under the red	7	2

Thus the blue glass produced an equality of heat in those two places where before it had always been different: indeed the prismatic red had been considerably weakened and rendered paler by it. In the preceding experiment with the violet glass, this had not been so strongly the case; for this glass gave itself a reddish violet image, which may be the cause why in that instance neither the effect nor the prismatic colour had suffered any change, although upon the whole the light was also weakened, as we may see from the small difference in the increase of heat in the spectrum above the temperature of the place.

[To be continued.]

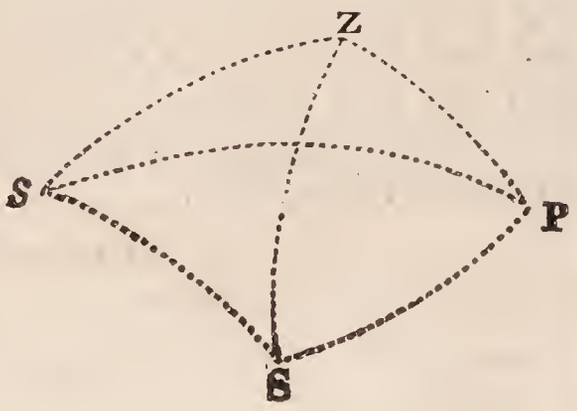
LV. *On*

LV. *Reply to the Observations of Mr.* Riddle *and Mr.* Henderson *on Mr.* Burns's *Mode of finding the Latitude by Double Altitudes. By* James Burns, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

THE remarks made by two of your Correspondents on my method for solving the problem of double altitudes seem to call for some reply on my part, as they both appear to have misunderstood the foundation and object of it. In the first place then, Mr. Riddle thinks that I "misapprehended the nature of the problem altogether," and asserts that I have assumed data, which, in nautical practice, cannot be obtained, viz. the horary angles or apparent time, and the *interval*. Now Mr. R. ought to have known that those angles can be always had, by means of the valuable Horary Tables first published by that distinguished astronomer M. Lalande in 1793; and he ought to have seen that my method *postulated* the use of such tables, which I distinctly stated in the outset. Hence it is very plain, that where those tables are used, the problem of double altitudes becomes a matter of mere curiosity, and that the importance which is attempted to be attached to it vanishes. However, the formulæ which I have given in my method will be the best verification of them, whether the observer would find his latitude by single or double altitudes. When I mentioned the chronometer, I did so (it must be evident to any one that understood the question) not for the purpose of deducing from it the apparent time, but merely to determine the interval. In this last point only, Mr. R. seemed to comprehend the proposed method. If the above-named tables were introduced into the modern *compilations* of navigation published in this country, it would be an essential improvement, as mariners would by their use be saved the trouble of practising the method of double altitudes, especially by rules which not one out of a hundred could practise, and not one out of a thousand could understand.—Besides, if there were no such tables in existence, the horary angles which, these gentlemen imagine, cannot be ascertained at sea, can be always had by calculation, as well as the latitude, as follows:



Let P be the pole, Z the zenith, S*s* the sun's parallel; and let us *suppose* PZ the co-latitude nearly known, as it generally will be; and then we have a spherical triangle,

of

of which the three sides are given to find *s*PZ. Again, with the polar distance augmented or diminished by the declination in the interval, we have another triangle SPZ, whose three sides are given to find ZPS. The sum or difference of these angles ought to equal the interval in degrees, &c.; and if it do not equal it, make PZ vary a little, and we should soon find a side PZ to represent the observations. Thus by a little trouble the apparent time may be determined, as well as the latitude. What Mr. Riddle means, when he calls the interval itself an assumption, in my solution, I own I am unable to divine, and must therefore leave to himself to clear up. Does he mean that the problem can be solved independently of the consideration of the interval? If so, it is plain that he has indeed "misapprehended" it.

It may be objected that the above method is indirect: but even so, it will be frequently found shorter than most of the methods that have been given; and the advantage of its determining the horary angles is peculiarly its own,—angles which these gentlemen seemed to think could not be ascertained.

Another method, which is direct, and in which the altitudes and the interval *only* are assumed, may be had, by means of the following formulæ, which it will be easy to deduce, from the properties of spherical triangles.

Let			
S P	=	a	$\sin.^2 \varphi = \sin. a \,.\, \sin. b \,.\, \cos.^2 \frac{m}{2}$
s P	=	b	$\sin. \frac{c}{2} = \sin. \left(\frac{a+b}{2} + \varphi\right) . \sin. \left(\frac{a+b}{2} - \varphi\right).$
S *s*	=	c	
Z *s*	=	d	$\sin. A = \frac{\sin. b \,.\, \sin. m}{\sin. c}$
Z S	=	e	
S P *s*	=	m*	$\sin.^2 \frac{B}{2} = \frac{\sin. \frac{1}{2}(d+c-c) \,.\, \sin. \frac{1}{2}(c+d-e)}{\sin. c \,.\, \sin. e}$
Z P	=	y	
P S *s*	=	A	$\sin.^2 \theta = \sin. a \,.\, \sin. c \,.\, \cos.^2 \frac{C}{2}.$
Z S *s*	=	B	
A $\pm$ B	=	C	$\sin. \frac{1}{2} y = \sin. \left(\frac{a+e}{2} + \theta\right) \sin. \left(\frac{a+e}{2} - \theta\right).$

in which φ, and θ, are what are denominated subsidiary angles. This method is *direct* and *rigorous*, and by means of it I ascertained that the latitude found by Dr. Brinkley's method was nearly correct;—but I also found that the interval given in the Doctor's example (2) was *incorrect;* and hence arose the great difference in the latitudes determined by his method and mine. And when the data are wrong or incongruous, it

* Equal the interval.

is no wonder that the calculations founded on them should be so. If this had struck Mr. R. or Mr. H., probably their critiques "would have been spared." It is very little to the purpose to tell us that some of the "greatest astronomers and mathematicians in Europe" would seem to merit censure for their proposed methods, if mine were unobjectionable: but no name, however great, can sanction error. Philosophers have long recognised the propriety of Horace's "Nullius addictus jurare in verba magistri." We could enumerate many of the greatest names in philosophy guilty of such errors. Newton, for example, in his investigations on the precession of the equinoxes, finds that part of it caused by the sun but 9″; while Euler, the greatest geometer, perhaps, of modern times, finds 22″, and D'Alembert 23″; that is, about $2\frac{1}{2}$ times the quantity found by Newton. And observation proves that the former were right.—But to return to our own simple question. I have said above, that the data in Dr. B.'s example are incongruous; and that they are so will easily appear, as follows. In latitude 27° 59′ at 2^h 55′ 12″ from noon, we ought to find, by the reverse problem, the sun's altitude 45° 6′, as in the example:—but what is the fact? The altitude will be found 46° 9′ at that moment; and instead of the altitude 5° 36′, we shall find the true altitude 6° 38′$\frac{1}{2}$. Hence the given *interval* is evidently incongruous, and the computations founded upon it must be false. It is unnecessary to dwell longer on this subject; but it was proper to show that both your correspondents misunderstood the foundation of my method, as well as some circumstances connected with the problem itself.

I remain, sir, yours, &c.

Hackney Road, Nov. 5, 1825. JAMES BURNS.

LVI. *Report of the Transactions of the Academy of Natural Sciences of Philadelphia during the Year* 1824; *submitted by the Recording Secretary, in pursuance of a Resolution of the Academy* *.

IN preparing the first annual report, your secretary has laboured under the disadvantage of having no precedent to guide him in his researches, and no means of ascertaining the objects which it was the wish of the Academy that he should

* We have been induced to reprint this Report, which has just been received in this country, from a conviction the perusal of it has given us, (with which we are sure our readers will coincide,) of the important advantages which the study of natural history will receive, and indeed has already received, from the zeal and activity of the association of our scientific brethren in the United States whose proceedings it records.—EDIT.

embrace

embrace in his report. In using his discretion upon this subject, he has chosen rather to include a number of points apparently of little interest, than to run the risk of condensing it too much by omitting subjects which ought to have been considered.

Whatever may have been the topics which a review of your minutes has suggested to his attention, all have presented themselves under an aspect which offers us great matter for congratulation. Whether we estimate the progress of this institution by the number of scientific communications submitted to it—by the number or merit of the memoirs deemed worthy of insertion in your journal—by the interest taken in your proceedings by the members themselves, as evinced in their more regular attendance at the meetings, and in the increased number of lectures delivered this year—by the accession to our list of associates—or, finally, by the improved state of our finances; we shall, in each of these bearings, discover great cause for rejoicing, and an assurance that our institution is daily increasing in importance, in respectability, and, what is still more desirable, in usefulness.

A few observations upon each of these points will constitute the chief part of this report.

The number of communications read before the Academy during the present year was thirty-seven; some of which, however, were so long as to require a division into many parts; in such a manner that scarcely has a meeting of the Academy taken place, during the present year, without the attention of its members being invited to the gratifying task of listening to the results of the observations or discoveries of one or more of their associates.

These communications may be classed as follows:

§ 1. *Zoology.*

Mammalia.—In this department the Academy received two communications: the first from Dr. Pœppig, entitled "Nova generis *Capromys* Species;" the second from Dr. Harlan, "On a species of Lamantin, resembling the *Manatus senegalensis* of Cuvier, inhabiting the coast of East Florida."

In his description Dr. Pœppig makes known to the scientific world a second species of that interesting genus of the *Rodentia*, which was first described by one of our naturalists under the name of *Isodon*. This name having been previously applied to another genus in Europe, Mr. Desmarest has substituted for it that of *Capromys*, and has changed the specific appellation of *pilorides*, which in justice to the first discoverer ought to have

been retained*. Dr. Pœppig's *Capromys prehensilis* is a valuable addition to our knowledge of those animals which were by Linné all united under the genus *Cavy;* but it has been, by some naturalists, supposed that Dr. Pœppig's animal ought to have constituted a new genus, distinct from the *Capromys*.

Mr. Cuvier, whose name bears with it so much weight in science, had, in his *Règne Animal*, divided the Lamantins into two species; thereby reducing the number of species, and clearing this subject of the perplexities in which it had been involved by the partial observations of former writers. The object of Dr. Harlan's communication is to show that the *Manatus senegalensis*, which had been supposed to exist only on the shores of Senegal, may also be an inhabitant of our territory. The specimens which Dr. Harlan received, and from which he prepared his descriptions, were, on the most accurate comparison, found to possess most of the characters by which Mr. Cuvier had distinguished the Senegal from the American Lamantin. Dr. Harlan has therefore arrived at the conclusion, that the *Manati* found on the coast of North and South America belong to two distinct species. Having ascertained this fact, he proceeds to state, that should further investigation and examination of the living animal from Florida prove it possessed of some external characters distinct from those of the *Manatus senegalensis*, then this animal will belong to a new species; for which Dr. Harlan suggests, as an appropriate name, that of *M. latirostris*.

Aves.—This department of zoology, which had hitherto occupied but a small space in the Journal of the Academy, has assumed a very interesting aspect from the labours of our new associate Mr. Charles Bonaparte, who is engaged in establishing a comparison between the observations of our celebrated naturalist Wilson and those of later European writers. This comparison will doubtless be highly beneficial to science, and will entitle Mr. Bonaparte to the thanks of all ornithologists, whether European or American. The communications which he has made to the Academy during the present year are as follows: 1st, "An account of four species of Stormy Petrels." 2nd. "On a new species of Duck, described by Wilson as the same with the *Anas fuligula* of Europe." And 3d, "Observations on the nomenclature of Wilson's Ornithology."

In the first of these, the author has investigated the Stormy

* Unless, indeed, as would appear from the observations of Mr. J. E. Gray, Mr. Desmarest's animal be different from that described by Mr. Say.

Petrels,

Petrels, which had not been clearly understood by former writers. He proves that there are four distinct species, three of which had been established by Temminck, whose names he of course retains; the fourth species, which our author admits, appears to be the same which Wilson had described as the *Procellaria pelagica:* it is an inhabitant of North America, but is seen at a considerable distance from our continent, as is proved by the fact of our author's having killed an individual of it near the Azores. This species being distinct from the *pelagica* of European writers, as well as from the other species of this genus, our author has applied to it "the name of *Wilsonii*, as a small testimony of respect to the memory of the author of the American Ornithology, whose loss science and America will long deplore." This species, besides its other distinctive characters, is easily recognised at first sight from its congeners by a yellow spot upon the membrane of the feet, a character which Mr. Bonaparte considers as permanent even in dried specimens.

Wilson had, in the eighth volume of his valuable work, described as the *Anas fuligula* of European authors a bird which appears to be distinct, and as yet unknown to European ornithologists. It ranges over the whole continent of North America, having been seen by Lewis and Clarke on Columbia river, by Mr. Say on the Missouri, and being occasionally met with in the Philadelphia market. Mr. Bonaparte describes it under the name of *A. rufitorques*. The crest of the *fuligula* offers its principal distinctive character, being very evident in this bird, while in the *rufitorques* it scarcely exists. The fine chesnut collar, the pied bill, and the beautiful lineations of the sides, are also distinctive characters of the male *rufitorques*. An important and constant distinction between the two birds, without reference to variations of sex or age, is the *speculum*, which in the *fuligula* is permanently white, while in the *rufitorques* it is invariably ash-coloured.

Mr. Bonaparte's principal memoir is undoubtedly that which has for its object to establish a concordance between the nomenclature used by Wilson and that of European ornithologists. This was a delicate and arduous task, of which a considerable part has been achieved. We shall refrain from any observations upon the results which Mr. Bonaparte has presented to the Academy, as these may be more advantageously considered after his task shall have been completed. In the mean while, the following quotation from his preliminary observations will show that in undertaking this task no disparagement of Wilson's talents or labour was intended: "Wilson, though one of the most accurate of ornithologists, one who

who has rendered the greatest services to science by describing in his attractive style the manners and habits of American birds, and who has corrected so many errors of former writers, has, nevertheless, unavoidably committed some himself, principally of nomenclature, which are in a great measure attributable to a want of the necessary books and opportunities of comparison. So far, therefore, from being censurable for these errors, we are surprised that he has not committed more."

Reptilia.—By the indefatigable zeal of Dr. Harlan and Mr. Say, this order of animals, which had previously attracted but a small share of the Academy's attention, has been the subject of six communications, five of which were offered by Dr. Harlan, and one by Mr. Say. They are entitled as follows: 1st, "Observations on the genus *Salamandra*, with the anatomy of the *S. gigantea* of Barton, or *S. alleghaniensis* of Michaux, and proposals for establishing two new genera." 2nd, "Description of two new species of the Linnæan *Lacerta*, with a proposal to form a new subgenus under the name of *Cyclura*, to include the *Cyclura carinata* and *C. teres*." 3rd, "Description of a new species of biped *Seps*." 4th, "Descriptions of two new species of *Agama*." 5th, "Description of a new species of *Scincus*." Of these five communications by Dr. Harlan, your secretary can offer no account, as the first has been withdrawn by the author, with the consent of the Academy, and the four others are still in the hands of the Committee of Examination.

Mr. Say's observations "On the fresh-water and land Tortoises of the United States" are highly important, as they tend to establish some order in a subject which has hitherto been very obscure. Mr. Say admits the existence in America of fifteen species of tortoises; viz. one of the genus *Testudo*, nine of the genus *Emys*, three species of *Cistuda*, one of *Chelonura*, and one species of the genus *Trionyx*. Of these, fourteen had been described by other authors, but in such a manner as to require revision and comparison. This our author has effectually done, and has added a new species of *Emys*, which he names the *biguttata*, whose distinctive characters are an oblong oval shell, slightly contracted in the middle, each side; anterior marginal scuta very narrow, linear; two large fulvous spots on the occiput; superior jaw emarginate; the inferior jaw acute; the tail rather long and simple. This is a small tortoise, which appears not to be common, as Mr. Say has seen but few individuals of it. It is an inhabitant of this vicinity.

Pisces.—This department has always been well cultivated in the Academy, owing to the valuable contributions of Mr. Lesueur,

Lesueur, whose exertions have furnished this year three interesting memoirs, of which the two first deserve particular notice; the third is still in the hands of the Committee of Examination. They are entitled, 1st, "Description of two new species of the genus *Batrachoid* of Lacépède." 2nd, "Description of several species of the Linnæan genus *Raia*." 3rd, "Description of two new species of *Blennius*." The genus *Batrachoides*, established by M. de Lacépède, has received from Mr. Lesueur considerable modifications. He discards from it the *B. blennoides* and *B. Gmelini*, included in it by Lacépède and Risso, and restricts it to the *B. Tau* of authors, the *B. vermuella* published by him in the *Annales du Museum d'Histoire Naturelle*, and to the two new species which he describes in this memoir under the names of *variegata* and *Diemensis*; the latter of which was formerly collected by him on Van Diemen's Land: the other has been recently found on our coast. Besides making known these two species, Mr. Lesueur's paper contains some general observations on the characters of this genus, which has not yet received much attention from naturalists, owing probably to the small number of species known.

The genus *Raia*, established by Linné, has been long since split into many genera, to accommodate all the species that were found to belong to it. The necessity of describing from dried specimens has doubtless been the cause of much of the uncertainty and confusion attending the descriptions given by authors of the different species of *Raia*. Mr. Lesueur has endeavoured to elucidate the point as far as relates to those of our country. He describes four new species, under the names of *Raia Desmarestia*, *Raia Chantenay*, *Trygon Sabina*, and *Myliobatis Freminvillii*; and makes it probable that, if the descriptions of Mr. Bosc are faithful, one additional species will exist, which for the present he unites to the *Raia eglantiera* of that author. Mr. Lesueur proceeds to describe an animal which, under the various absurd appellations of Devil Fish, Wonderful Sea Serpent, and Vampyre of the Ocean, had attracted considerable attention, and had been exhibited in several of our cities. He describes it as a *Cephaloptera Giorna*. This is probably the same animal which Dr. Mitchill, of New York, had described in the Annals of the Lyceum of Natural History of New York, under the name of *Cephalopterus Vampyrus*. After a number of practical observations on the Rays, exhibiting an intimate acquaintance with his subject, and the real value of which can be fully appretiated by those alone who are conversant with ichthyology, Mr. Lesueur adds, "I have adopted for this species the name of the celebrated Giorna,

well

well known in science, and reject such names as Devil, Vampyre, &c., which may well be associated together, as names calculated to repel those who are disposed to admire the beauties of nature and who have an inclination to cultivate scientific natural history. How far preferable is the custom of applying the names of those naturalists who have enriched science with new discoveries, or new and valuable observations, to that which introduces into our pages those chimeras that do not elsewhere exist than in a morbid or timorous imagination?"

It has frequently happened that, from the great scarcity or shyness of certain animals, they have remained undiscovered for a length of time, even in spots to which naturalists have frequently resorted. Of this we find a new instance in the discovery lately made known to the Academy by Mr. Gilliams, who has described a very beautiful species of *Scolopsis* caught by him in the neighbourhood of this city. Mr. Gilliams has as yet seen but three individuals of this species, which he has dedicated to his friend Mr. Say.

The last communication in ichthyology which the Academy received was from Mr. William W. Wood, and is entitled "Descriptions of four new species of the Linnæan genus *Blennius*, and of a new *Exocetus*." This paper has not yet been reported upon to the Academy: we therefore refrain from making any comments upon its objects, but we may congratulate the Academy upon the first scientific production of Mr. Wood, who, notwithstanding his youth, has cultivated science with such zeal and success, as to give promise of great usefulness. Though not yet a member, we may, in a measure, claim for this institution the merit of having formed this young naturalist, since it is chiefly by intercourse with our members, access to our library, and attendance at our meetings, that his taste and talent for science have been developed.

Insecta.—Mr. Say has continued, during the last year, his useful investigations of American insects. The reputation which he has acquired, both at home and abroad, as the most correct American entomologist, imparts great value to every thing which the Academy receives at his hands. A long communication, received from him this year, includes the "Descriptions of the new species of Hemipterous insects, collected during the expedition to the Rocky Mountains." This expedition, so useful by the information which it has furnished in all the departments of science, enabled Mr. Say to discover hundreds of insects, inhabitants of our country, and which were altogether unknown to naturalists. Of the family of the *Hemiptera* alone we have forty new species. This paper is as yet unpublished.

Mollusca.—

Mollusca.—This department has already been enriched by valuable papers from Mr. Lesueur; and in the last year he has added to his former observations, by his "Description of a new species of *Cephalopoda*, of the genus *Loligo*." The Loligos have been so fully studied, and the number of their species so much increased by this gentleman, that he may almost be considered as having appropriated to himself this genus of Lamarck's. His last communication makes us acquainted with an inhabitant of our bay, which has received from him the name of *L. brevipenna*. The individual which our author observed, appeared to him to resemble the genus *Sepiola* more closely than any other Loligo which he had seen. In the same communication Mr. Lesueur adverts to the fact of his having, in the year 1814, observed a species of *Sepiola* in the British Channel. This will prevent our erroneously drawing an inference of their non-existence in those waters, from the statement made by Mr. Blainville that he had never met with them there.

Vermes.—Mr. Lesueur has described "three new species of parasitic Vermes, belonging to the Linnæan genus *Lernæa*," which he refers to Mr. Blainville's genera *Lerneocera* and *Lerneopenna*. In order that two of his species, the *cruciata* and *radiata*, may be included in the former genus, its generic characters must be extended so as to admit species whose arms are simple and not branched. Should this modification of the *Lerneocera* not be admitted, then Mr. Lesueur's two species will constitute a new genus, for which he suggests the name of *Lernecœnicus*. The third species which he describes is the *Lerneopenna Blainvillii* which inhabits the *Exocetus volitans*.

Zoophytes.—The same able naturalist has contributed to this department by his investigations of the *Holothuria*, from the island of St. Bartholomew. In his classification of these animals, Mr. Lesueur has been led to prefer divisions founded upon modifications in the form and structure of the tentacula, to those which depend upon the correspondencies of form and disposition of feet, as had been adopted by Messieurs de Blainville and Cuvier.

Availing himself of the two divisions adopted by Mr. de Lamarck, our author suggests that those species which have pinnated tentacula should be placed in a distinct division. Of these, Mr. Lamarck was acquainted with only one species. Mr. Lesueur's descriptions include four species with cylindrical tentacula (*H. obscura*, *agglutinata*, *muculata*, and *fasciata*), two species with arborescent tentacula (*H. lapidifera* and *Briareus*), and two with pinnated tentacula (*H. hydriformis* and *viridis*).

In addition to the above communications on zoology, a paper

was received from Mr. Jacob Cist of Wilkesbarre, describing a singular *lusus naturæ*. This paper, though valuable, was considered as not coming within the general plan of the Academy's journal; but it has since been published in another journal, whose design was more comprehensive.

[To be continued.]

LVII. *Reply to Mr.* Davies's "*Further Thoughts on Mr.* Herapath's *Demonstration.*" *By* P. Q.

IT is really difficult to say which is the most striking in Mr. Davies's criticism—the skill and ingenuity of his attack, or the honourable candour and liberality which he displays towards the party opposed. We may with propriety observe, that "*intaminatis fulget honoribus.*"

If in the observation, "whether Mr. Herapath has failed in his usual precision of expression," Mr. Davies means to imply that Mr. H. has written with a little too much brevity for perfect elementary perspicuity, he is probably not far from the truth. But this ought not to be charged as a fault so much on Mr. Herapath as on the limits to which he was of necessity confined. It would have been impossible to comprise so many points, as he has in that paper treated of, within any reasonable bounds, had he not in every part studied the utmost compression.

As Mr. Davies has transferred his attack from the previously contested points to others, I shall in my defence of Mr. H. follow his example, and try if I cannot convince that gentleman, without "any new species of 'mathematical magic,'" that Mr. Herapath is here too equally as invulnerable as in the positions abandoned. Mr. H. assumes

$$r + v = n, \text{ any integer whatever,}$$

and tells us that "r *or* v *may be any number rational, irrational, or imaginary;*" a fact, the truth of which is so obvious that any one would smile at an attempt to establish it by demonstration. Mr. Herapath then adds: "*and since the sum* n *of these numbers*" (r and v) "*is an indeterminate positive integer, they will in point of value be independent.*" That this, which is one of the disputed points, is correct, may be shown in an instant. For suppose one of the quantities, r for instance, at the time it has any non-integral value to remain constant, whilst n varies any how through integral values only; then

$$\Delta v \text{ is evidently } = \Delta n,$$

v still retaining its non-integral value. That is, v is a non-integer and a variable, whose variations are not necessarily at all

all connected with the value of r, or in any ways dependent on it; which is all that Mr. Herapath appears to mean by saying, the two quantities are "independent in point of value." Probably, had Mr. H. designated these quantities "independent variables," as he does just below, it would have been more to the point; though he has committed no error by a different denomination, in the sense which he has evidently employed. "Mathematical magic" need not therefore be called in to establish Mr. Herapath's position: but what except "magic" of some kind could induce Mr. Davies to draw a comparison between the relation of r and v, whose sum is an *indeterminate* integer, and that of an angle and its complement, whose sum is an *invariable* quantity, I do not know. The variations of r and v, I have already proved, may, by giving to n an indeterminate value, have no relation: but it would require "mathematical magic" indeed, to prove the same of the variations of "an angle and its complement, or of a number and its reciprocal:" the variations of which quantities have fixed, or at least definable relations.

Having demonstrated the absolute independence of the variations of r and v, I shall proceed to prove the remaining part of Mr. Herapath's arguments on much more general principles than he has done. This will give me an opportunity of introducing a theorem in functions of no contemptible importance, which I have never met with elsewhere; but which there can be no doubt Mr. H. had his eye full upon when he composed his demonstration.

Theorem.

Let p_r, q_v *be any two functions of* r *and* v *respectively; and let*

$$f(r, v) = \mathrm{F}(p_r, q_v) \qquad (1)$$

Then, if the changes of r, v *be independent the one of the other; and if* $f_1 r$ *be the function in the left-hand member of* (1) *containing* r *only,* $f_2 v$ *the function containing* v *only, and* $f_3(r, v)$ *the function containing those terms only which have* r *and* v *mutually combined; and if* $\mathrm{F}_1 p_r$, $\mathrm{F}_2 q_r$, $\mathrm{F}_3(p_r, q_r)$ *are like functions in the right-hand member; I say that, besides equation* (1), *the three following will subsist simultaneously,*

$$f_1 r = \mathrm{F}_1 p_r \qquad (2)$$

$$f_2 v = \mathrm{F}_2 q_v \qquad (3)$$

$$f_3(r, v) = \mathrm{F}_3(p_r, q_v) \qquad (4)$$

For since the changes of r and v are independent, we may suppose one of them, r for example, constant; while the other varies. Regarding therefore (1) as functions of the variable v only, and imagining these functions developed according to the powers of v, each side must contain identically the same

terms. If not, by transposition we should have an algebraic equation of v and constants, which would of course limit this quantity to an invariable value, and thus make it a constant, against the hypothesis. But in these developments of (1), $f_1 r$, $\mathrm{F}_1 p_r$ are the whole of the functions which contain r only; and consequently by the property of identity just proved

$$f_1 r = \mathrm{F}_1 p_r$$

By similar reasoning it follows that

$$f_2 v = \mathrm{F}_2 q_v, \text{ and } f_3 (r, v) = \mathrm{F}_3 (p_r, q_v).$$

It should here be observed that the function $\mathrm{F}(p_r, q_v)$ is not confined to powers and products of p_r and q_v; for it may have the more general signification of

$$\mathrm{F}\{p(r), p_1(r), p_2(r), \ldots, q(v), q_1(v), q_2(v), \ldots\}$$

in which p, p_1, p_2, ... and q, q_1, q_2, ... are all different functions of r and v respectively, though for brevity $p(r)$ and $q(v)$ only are expressed.

Hence the truth of Mr. Herapath's proof is apparent. I could have easily established the above theorem by putting r and v separately $= 0$, and not in the least affect the legitimacy of Mr. H.'s arguments. But apprehending Mr. Davies might think he had reason for playing against such a procedure the surprising inferences (Phil. Mag. for October, p. 276) he has drawn on the supposition of r and v being put separately $= 0$, I was willing to show that our resources are not so meagre as to depend exclusively on a solitary process.

It is now manifest that Mr. Herapath's demonstration is correct and complete, whether the exponents be real or imaginary. Of this I hope Mr. Davies will be convinced; and if he be, I am persuaded, from the honourable and manly sentiments he has displayed, that he will not be backward in acknowledging it. Should he however not be satisfied, it will be incumbent on him to show by actual examples the fallacy of the preceding theorem, which comprehends the principles of Mr. Herapath's process.

Mr. Davies complains of my having wrongfully charged him with a wish to overturn most of Mr. Herapath's mathematical labours. From the general tenour of that gentleman's first paper, and his particular allusion (Phil. Mag. for Aug. pp. 117, 118) to Mr. Herapath's "reasoning on periodical functions," which does certainly not at all depend on the binomial demonstration, I did imagine Mr. Davies intended a sweeping objection to the whole or the greater part of Mr. Herapath's mathematical writings; and I conceive almost any other individual would think so too on reading the paper in question. However,

ever, if I have misunderstood Mr. Davies, I beg to offer him my best apologies, and to assure him I had no intention of laying a greater stress on his language than I thought it was intended to convey.

P. Q.

LVIII. *Researches on the Composition of Peridot.* By L. P. Walmstedt.

SINCE the application of the electro-chemical theory and of the doctrine of chemical proportions to mineralogy, a far surer criterion of the greater or less accuracy of chemical analyses has been gained; and it has consequently been found that some minerals with whose composition we seemed previously to have been pretty well acquainted, require in fact a new examination. Among these is peridot.

It is well known that Haüy upon crystallographic grounds united into one species the two fossils separated by Werner, (chrysolite and olivine), and named them peridot. This decision of crystallography ought first to receive its full confirmation from chemistry; for, with the exception of Haüy's regular bodies (as they are called), there has not yet been found to exist a similarity of primitive form with *essential* differences in the component parts. This confirmation is in vain sought for in the specimens of peridot at present known, which rather evince the contrary; for, if we except Achard's analysis of the chrysolite, and Gmelin's examination of the olivine, the results of which entitle us to suppose that the minerals they had in their hands were very different from what they named them, all the other known specimens correspond in some degree with the formula $\left.\begin{matrix}M\\F\end{matrix}\right\}S$ for chrysolite, and $\left.\begin{matrix}M\\F\end{matrix}\right\}S1\frac{1}{2}$ or $\left.\begin{matrix}M^2\\F^2\end{matrix}\right\}S^3$ for olivine.

I undertook the following investigation, which I submit to the intelligent judgement of the Royal Academy, partly on account of the contradiction between theory and analysis, partly for the sake of ascertaining the real nature of the mineral in the meteoric iron of Pallas which resembles olivine.

As the small number of the different peridots which I had at command did not always permit a repetition of the analysis, I was obliged, in each experiment, to pursue the same course, in order to make the results more correspondent. After the washed powder of the stone was dried for about ten minutes in a small apparatus filled with carbonic acid gas, to prevent the further oxidation of the iron from the fire, which was at the point of ignition, between one and two grammes were weighed out for analysis. The stone was fused with four times its

its weight of carbonate of potash. Of the purity of the silica, as it is usually separated, I convinced myself by repeatedly fusing it with potash: I renewed its separation and conversion to fluidity, and treated it with caustic ammonia, which only occasioned a slight flocky precipitate, and this probably resulted only from the corrosion of the filter. And when phosphate of soda was added to it, not the least trace of a precipitate appeared.

I now boiled the very acid solution, which had been separated from the silica and was of a greenish yellow colour, for an hour with nitric acid, precipitated it with a little caustic ammonia, boiled the dark-red precipitate half an hour in caustic alkali, and from the alkaline solution mixed with muriatic acid I precipitated by carbonate of ammonia some inconsiderable flakes of alumina. The remainder which had not been taken up in the caustic alkali was dissolved in muriatic acid, and the iron, after a more perfect neutralization, precipitated by means of succinate of soda. When I poured over the precipitate, after it had been well washed, weak solution of ammonia, and then evaporated it by a gentle heat, I could not once remark that the platina crucible had been attacked. On re-dissolving the oxide of iron in muriatic acid, there was a residuum for the most part of one and even two milligrammes of gelatinous silica.

The colourless fluid which had been separated I now mixed with some drops of oxalate of potash and ammonia, but was unable, even after it had been digesting many days on a warm stove, to discover any trace of a precipitate. Peridot is, however, not free from lime. I next united the solution with the fluid that had been taken from the succinate of iron, precipitated the magnesia with carbonate of potash, according to the manner laid down by Bonsdorff, ascertained the weight of the earth which was ignited, and again dissolved it in muriatic acid, when some fumes of chlorine were disengaged, but this not till towards the conclusion. After the distillation, till I dried it and dissolved it in water acidulated with muriatic acid, silica remained, which never weighed more than 0·6 per cent, and often much less. I now mixed the solution with hydro-sulphuret of ammonia, again dissolved the dark-coloured precipitate in muriatic acid, and at last precipitated it by boiling it with carbonate of potash. As the hydro-sulphuret of ammonia contained a solution of magnesia, I mixed it with sulphuric acid till the existing combinations were completely destroyed; I then dried it by evaporation, and heated the residuum till the excess of sulphuric acid had disappeared. As the mass of salt was dissolved by a concentrated solution of gypsum, no doubt remained of the presence of lime, while a small,

a small, and sometimes a merely impalpable trace of gypsum remained undissolved. I must still remark that by this process I ascertained the presence of lime only in the varieties of the peridot, as will afterwards be shown. In accordance with the experiment just described, the following peridots were analysed.

Olivine from the top of a mountain in Silesia which is covered with snow.

The analysis made with 1·705 gr. as stated, of the dried pulverized stone, produced

			Oxygen.
Silica	41·54		21·60
Magnesia	50·04	19·37	= 21·34
Protoxide of iron	8·66	1·97	
Protoxide of manganese	0·25		
Alumina	0·06		
	100·55		

Olivine from Bohemia.

The analysis made with 1·292 gr.

			Oxygen.
Silica	41·42		21·54
Magnesia	49·61	19·20	= 21·28
Protoxide of iron	9·14	2·08	
Protoxide of manganese	0·15		
Alumina	0·15		
	100·47		

Olivine from the district le Puys in the Vivarais.

For the experiment 1·601 gr. were used.

			Oxygen.
Silica	41·44		21·55
Magnesia	49·19	19·04	= 21·25
Protoxide of iron	9·72	2·21	
Protoxide of manganese	0·13		
Lime	0·21		
Alumina	0·16		
	100·85		

The mineral resembling olivine in the meteoric iron of Pallas.

For the determination of its specific gravity a pure piece weighing 1·5585 was chosen; the specific gravity was at 62·6 F. = 3·362.

Silica

			Oxygen.
Silica	40·83		21·23
Magnesia	47·74	18·48	= 21·11
Protoxide of iron	11·53	2·63	
Protoxide of manganese	0·29		
Traces of lime			
Traces of alumina			
	100·39		

Olivine from Somma.

Two quantities of this were analysed, one consisting of 1·684 gr., the other of 1·9435 gr., and the following are the results:

	1st Anal.		Oxygen.	2nd Anal.		Oxygen.
Silica	40·08		20·84	40·16		21·88
Magnesia	44·24	17·13	= 20·60	44·87	17·37	= 21·87
Protoxide of iron	15·26	3·47		15·38	3·50	
Protoxide of manganese	0·48			0·10		
Alumina	0·18			0·10		
	100·24			100·61		

According to these analyses, the composition of olivine may very well be expressed by the formula $\left.\begin{matrix}\mathrm{M}\\ \mathrm{F}\end{matrix}\right\}\ddot{\mathrm{S}}$ or $\dot{\mathrm{R}}^3\,\dddot{S}i^2$, supposing that R indicates the class of isomorphous bodies to which magnesia and protoxide of iron belong. If we further compare with this the known analyses of chrysolite, which, as I stated, correspond with the same formula, it seems established also by chemistry that chrysolite and olivine must be considered one and the same mineral, their chemical relations being the same, and their integral nature being susceptible of expression by the formula which I have proposed.

It is well known that olivine often has a marked tendency to disintegrate. In order to trace the cause of this more closely I examined some disintegrated olivine from the top of the Wilhelm at Cassel. Its colour was a rusty yellow, its lustre and transparency had disappeared; but its cohesion, although diminished, still prevented it from crumbling to powder. In the interior of one part of the grains the disintegration had not proceeded so far; so that the colour, although it was already turning very much to yellow, still preserved a mixture of green: there might also be perceived more or less transparency still, as well as lustre, but this was changing to a dirty appearance.

The

The analysis was made with 1·8985 gr., and gave the following results:

			Oxygen.
Silica	42·61		22·16
Magnesia	48·36	18·91	= 20·81
Protoxide of iron	8·86	1·90	
Protoxide of manganese	0·15		
Lime	0·22		
Alumina	0·14		
	100·34		

According to this result the presence of an alkali could not be conjectured; yet as the presence of alkali often contributes to the disintegration of minerals exposed to the weather, I examined the olivine in respect to this particular, and took a piece, of 1·901 gr., which was a little weathered. The process consisted as usual in igniting it with carbonate of barytes, separating the silica, and precipitating the solution with sulphuric acid and caustic ammonia. The fluid separated from the precipitate was evaporated in a platina capsule and heated to dryness. On dissolving the dry mass a little mixture of a grayish earth remained, which was separated, and the solution again evaporated. The remaining white salt, weighing 0·155 gr., did not fuse with a low red heat: it readily dissolved in water, and by spontaneous evaporation it produced long acicular crystals which showed no tendency to be altered by the weather. As it hence followed that the greatest part of it consisted of sulphate of magnesia, I again dissolved it in water: a few spiculæ of gypsum then remained behind; and I decomposed it with acetate of barytes, then evaporated the fluid which had been freed from the precipitate, ignited the residuum, and deprived it of salt by boiling water. After the evaporation of the filtered fluid and its ignition in a suitable platina crucible, its weight had increased only about 0·0005 gr. On closer examination the cause of this appeared to be some earthy stains at the bottom, which were not changed by water, and which, without doubt, proceeded from a little residuum of magnesia. According to this examination no alkali could have existed in this olivine.

In regard to the change which the olivine suffers from the influence of the weather, from the mere appearance of it, it may be concluded that the protoxide of iron passes thereby into peroxide. According to the analysis also, the quantity of iron in it is less than in the other kinds of peridot; and on the other hand there is an excess of about 2 per cent of silica, which seems to proceed from a diminished proportion of magnesia. From this it might be concluded, that after the chemical connexion of the constituent parts had been destroyed

by the increased oxidation of the iron, the quantity of magnesia was gradually lessened by the influence of atmospheric moisture (*meteorwasser*), and that the final result of the decay of the fossil would probably be a mixture of oxide of iron, perhaps as hydrate, and silica; yet I venture to attribute no great weight to this examination, partly because I was able to decompose only *one* decayed olivine, and partly because the opacity of the piece examined rendered it impossible for me to convince myself as I wished, of its purity.

There still remains, in every view of the case, the question, In what does the cause of the olivine's great tendency to yield to the influence of the atmosphere consist? for there are well-known minerals with a greater proportion of protoxide of iron, which are not distinguished by such a disposition to decompose. I should seek it in the granular texture of the mineral, and the greater penetrability which it consequently has in relation to air and water; for chrysolite, which displays no granular texture, has not this tendency. Hence it would merit a closer investigation, whether the olivine is not less liable to such decay if its granular texture is less distinct, and thereby approaches more to that of chrysolite.

Appendix.—To the foregoing investigation succeeds an interesting extract from the treatise read by our distinguished analyst Professor Stromeyer, in the Göttingen Society, at the last celebration of their anniversary, entitled *De Olivini, Chrysolithi et fossilis, quod cellulas et cavernulas ferri meteorici Pallasii explet, analysi chemicâ.* The great resemblance which the mineral that occurs in the Pallas iron and fills its cavities has in its exterior to olivine and chrysolite, has long occasioned the conjecture that it also did not differ from these minerals in its chemical constitution, and that it belongs with them to one and the same mineral species. If, however, we compare the analyses of it communicated by Howard and Klaproth, with the results which the decompositions of the olivine and chrysolite by Klaproth and Vauquelin have given, this opinion will still seem very doubtful. According to these experiments, indeed, the mineral from the Pallas iron consists of the same component parts of which olivine and chrysolite are composed, and is, like these, chiefly formed of silica, magnesia, and oxide of iron. But the proportion in which these substances enter into its composition differs from that in which they occur in olivine and chrysolite too much for it to be maintained that it is perfectly identical with them. Yet as the experiments of Howard do not correspond with those of Klaproth, and as also the olivine in its composition does not correspond according to those analyses in regard to the *quantitative* relation, it was quite necessary that these minerals generally should be submitted

mitted afresh to a careful examination; at the same time that the more accurate knowledge of their constituents is so nearly related to the researches concerning the chemical constitution of meteoric stones; for, as is well known, the basis of most of these remarkable substances seems to consist of a mineral similar to the olivine species.—Engaged in an express chemical investigation of meteoric iron and meteoric stones, Professor Stromeyer found also occasion to attend to this object, and now laid before the Royal Society of Göttingen, in the treatise which has been mentioned, his analyses of these minerals.

First, the investigations relating to the olivine are communicated. For these, an extremely pure and perfectly undecayed olivine from the basalts of the Vogelberg at Giessen was used in preference; for which Professor Stromeyer was indebted to the kindness of Professor Wernekinck at Giessen and Dr. Thilenius at Weilburg. Its specific gravity was found to be, in one experiment at 45·5° F. and barom. 29·095 in. = 3·3324; and in another experiment at 67·55° F. and barom. 29·410 in. = 3·3386.

According to the mean of three analyses conducted with the greatest care, and differing from one another very inconsiderably, this olivine seems to be composed of

Silica	40·09
Magnesia	50·49
Protoxide of iron	8·17
Oxide of nickel	0·37
Oxide of manganese	0·20
Alumina	0·19
	99·51

As, however, the component proportions of the olivine in this analysis differ very considerably from the result which Klaproth obtained, this investigation was repeated with another olivine from Bohemia, just as pure and well preserved, which occurs in the basalt at Casalthof.

The analysis of this produced a result perfectly corresponding with the former.

From 100 parts of this Bohemian olivine, whose specific gravity was at 49·1° F. and barom. 29·646 in. = 3·3445, were extracted

Silica	40·45
Magnesia	50·67
Protoxide of iron	8·07
Oxide of nickel	0·33
Oxide of manganese	0·18
Alumina	0·19
	99·89

The analysis of the olivine by Klaproth, therefore, states the oxide of iron and the silica contained in this fossil much too high, and on the other hand the quantity of the magnesia at least 12 per cent too low. From the process pursued by this chemist, it is easy to perceive how so considerable a proportion of magnesia might have escaped him, and how the silica and oxide of iron must thereby have appeared so much the greater in quantity.

According to the experiments of the same chemist, some lime is also contained in olivine. But neither in the olivine from the Vogelberg, nor in that from Casalthof in Bohemia, was there any trace of it; and as this substance was in vain sought for in many other olivines also, and particularly in that from the Habichtswald at Cassel, whence that examined by Klaproth was brought, it may very well be assumed that the lime obtained by him resulted from the mixture of another calcareous mineral accidentally with this, or perhaps might have arisen from the filtering paper, which at that time it was not customary to purify previously by acids.

The discovery of oxide of nickel in olivine is a new fact, which, notwithstanding the small quantity in which this metallic oxide enters into the composition of the mineral, is still of importance in regard to its origin. As the presence of oxide of nickel in olivine had not till now been observed by any chemist, the suspicion at first naturally arose that it was contained merely accidentally in the olivines submitted to this specific examination. But experiments which on this account have been made with many other very pure olivines, brought from very different parts, (as for example, with that of the Habichtswald, that from the Eifel, from Vesuvius, from Rantieres at Ardes in Auvergne,) leave no doubt of the constant presence of oxide of nickel in this mineral.

The quantity of oxide of nickel found in olivine now induced the suspicion of the presence of oxide of chrome in it also; but neither by treating it with nitre, nor by dissolving it by means of caustic alkali, could any trace of this metallic oxide be detected.

The iron occurs in the olivine, as is already mentioned, in the state of protoxide; and by no means as black oxide of iron, as is assumed by Klaproth. However, there is still found in it also a slight mixture of this metal in the state of black oxide. To this, olivine owes its pale yellowish-green. By continued ignition, with the admission of the air, this combination passed to black oxide of iron; and then it displays similar variegations of colour to those of iron window frames, where this phænomenon also proceeds from an increased

creased oxidation of the metal. On fusing this mineral before Marcet's lamp, all the protoxide of iron at length passes into black oxide; hence also the globule obtained assumes a dingy-black colour.

The analysis of the chrysolite succeeds to this.

Professor Blumenbach was so kind as to permit Professor Stromeyer to avail himself of two rough chrysolites for that purpose from his collection, which exhibited not only distinct crystalline planes, but also possessed all the other characteristics of pure chrysolite. Their specific gravity, according to a mean of three weighings, was at 45·5° F. and barom. 29·016 in. = 3·3514, which differs very insignificantly from the specific gravity found in olivine. According to the experiments made upon it, this mineral corresponds also in its component parts and in its chemical properties with olivine, and consists not only of the same integral parts, but contains these also exactly in the same proportions as those of olivine. The quantity of iron alone is in chrysolite somewhat greater than in the two kinds of olivine that were examined.

In 100 parts of chrysolite submitted to this analysis were contained

Silica	39·73
Magnesia	50·13
Protoxide of iron	9·19
Oxide of nickel	0·32
Oxide of manganese	0·09
Alumina	0·22
	99·68

With this result, as to the chief object, the analysis of the chrysolite by Vauquelin also very well corresponds; as, on the contrary, Klaproth's differs from it very considerably.

After the conclusion of the analysis of olivine and chrysolite, the olivine-like mineral of Pallas was likewise submitted to an equally careful examination. The opportunity of undertaking an exact chemical analysis of this extremely rare fossil, Professor Stromeyer owed to Dr. Blumenbach and Dr. Chladni, who both, with a kindness that surpassed his wants, provided him with this rare mineral substance. In order as much as possible, to avoid all error in this investigation, only perfect and pure grains of this fossil were used: they were all selected with the utmost care with the help of a lens; and neither particles from the mass of iron, nor from the rust into which the iron had been partially converted, were permitted to contaminate them. The specific gravity of these pure grains was at 68° F. and barom. 29·410 in. = 3·3404, which very exactly corresponds with that of olivine and chrysolite.

According

According to an average of three analyses of this mineral, which all agreed very well with each other, it was found to be composed, in 100 parts, of

Silica	38·48
Magnesia	48·42
Protoxide of iron	11·19
Oxide of manganese	0·34
Alumina	0·18
	98·61

Its composition is therefore wholly the same as that of olivine and chrysolite. But the quantity of protoxide of iron is somewhat greater per cent than in these two minerals; and, what is very remarkable, the oxide of nickel is wholly absent. Howard, indeed, says he found some in it: but as this chemist does not state that he separated the olivine from the mass of iron with all possible care, it is easy to conceive that from his experiment nothing can with certainty be concluded; and besides, his datum of a quantity of oxide of nickel is no refutation of the above experiments.

As, from these researches, it is now clear that olivine, chrysolite, and the mineral resembling olivine from the Pallas iron, have the very same components, it is no longer a subject of any doubt that these three minerals belong to one and the same species, and can be distinguished from one another only in regard to their appearance.

As, moreover, the proportion of the silica to the magnesia is the same in the three minerals, and they exactly correspond in the proportions of their equivalents, which compose $\frac{9}{10}$ths of the whole, while the iron alone varies; so it is, consequently, very probable that the silica in them is combined only with the magnesia, and that this silicate of magnesia is also their only essential constituent. On the other hand, the protoxide of iron with the oxide of nickel, the oxide of manganese, and the aluminous earth, appear to be merely diffused in this silicate.

That no oxide of nickel should be contained in the Pallas mass, although it occurs in an iron which so clearly contains nickel, is certainly at the first view extremely strange. If, however, it is supposed that this mass of meteoric iron existed in a fused state, and at the same time the ready reducibility of the oxide of nickel and its slight congeniality for siliceous combination is kept in view, it is not improbable that these circumstances have prevented the admission of this metallic oxide into the Pallas iron; just as is the case with the smalt, where, notwithstanding the use of niccoliferous cobalt ore, the oxide of cobalt alone, with a mixture of iron and arsenic, combines with the vitreous flux, while the nickel becomes separated

parated in the metallic residuum. The formation of olivine, on the other hand, manifestly took place under the co-agency of water, and therefore under circumstances favourable to the combination of this metallic oxide. Perhaps it is also hence not improbable that the oxide of nickel was taken by the olivine from the gangue, and it might therefore not be uninteresting to examine the basalt and the basaltic tufa to ascertain the presence of nickel. The circumstance also, that oxide of nickel likewise occurs in the chrysolite may, according to this, very well accredit the conjecture that this mineral (whose true habitat and peculiar appearance we are not yet acquainted with) exists also in basalts, and by no means has a meteoric origin.

The analysis of two other fossils of the olivine kind, taken from two more masses of meteoric iron, concludes this treatise.

One of these minerals is met with in a ramous mass of iron very like that of Pallas, which was found at Otumpa in the province of Chaco-Gualamba, in South America; and a piece of which the illustrious M. Von Struve of Hamburg was so kind as to send to Professor Stromeyer for chemical examination.

The olivine of this iron is externally like that which was taken from the Pallas iron as the most perfect specimen. Its specific gravity is at 68° F. and barom. 29·429 in. = 3·3497. And in its composition it also exactly corresponds with the Pallas olivine. In 100 parts of it are contained

Silica	38·25
Magnesia	49·68
Protoxide of iron	11·75
Oxide of manganese	0·11
	99·79

The second of these two minerals, said to have been found in the district of Grimma in Saxony, is part of the meteoric iron which is preserved at Gotha in the Ducal Cabinet, and was about a hundred years since contained in the collection of minerals purchased for the cabinet of the former Saxon burgomaster of Schenberg. Yet by the permission of the late duke, Professor Stromeyer obtained through the kindness of the Chancellor Braun in Gotha many fragments of this iron, and also two grammes of the olivine with them.

This olivine externally is not materially different from that of the other masses of meteoric iron. Its specific gravity is however somewhat less, and is, at 72·5° F. and barom. 29·764 in. only = 3·2759. But in its constituents it wholly differs from both the foregoing ones.

According to two analyses of it, which corresponded very well with one another in the chief points, there are contained in 100 parts of this mineral Silica

Silica	61·88
Magnesia	25·83
Protoxide of iron	9·12
Oxide of manganese	9·31
Oxide of chrome	0·33
Loss by ignition	0·45
	106·92—[?]

This mineral consequently contains for three equivalents of silica only one of magnesia, and is therefore to be considered as a trisilicate of magnesia; while the mineral of the olivine kind, from the Siberian and South American masses, as well as the olivine from the basalts, and chrysolite, are merely a simple silicate of magnesia. Whether, moreover, the oxide of chrome that occurs in it really belongs to its composition, or is only accidentally contained in it as an alloy of chrome and iron, cannot yet be decided by these experiments.

The discovery of this properly olivinic mineral in the meteoric iron at Gotha not only affords a proof of the difference of this iron from that of Pallas, but is certainly important also in regard to the knowledge of meteoric stones particularly; because, according to the silica and magnesia which is found in it, it is not improbable that this very kind of olivine forms pre-eminently the basis of meteorites.

Note.—To the concluding remark of Professor Stromeyer may be added a memorandum of Humboldt on a remarkable aërolite, a notice very interesting in respect to the science of meteoric stones, but still indeed very incomplete.

At the session of the Pharmaceutic Society at Paris, on the 16th May, Baron Humboldt announced that there had been found an aërolite which is really a volcanic production, because it consists of crystals of augite. It remains still doubtful whether it was ejected from a lunar volcano.

LIX. *A Letter from M.* Ampere *to M.* Gerhardi *on various Electro-dynamic Phænomena.**

I BEG to return you many thanks for the copy of your observations upon the work of the Chev. L. Nobili which you were kind enough to send me.

Your answers to several objections contained in that work against some parts of my theory of electro-dynamic phænomena appear to me in general very just, and are indeed in my opinion quite satisfactory. They had for the most part struck

* From the *Annales de Chimie*, tom. xxix. p. 373.

me

me when I read M. Nobili's work; in which, moreover, there are researches upon various circumstances of electro-dynamic phænomena which abound in interest.

You have plainly shown, sir, that the result of all the experiments described in that work entirely agrees with that deducible from my mode of explaining electro-dynamic phænomena. I must, however, add two observations to those you have already made on that subject. The first relates to my having asserted in a letter to Mr. Faraday dated April 18th, 1823, that the mutual action of two complete circuits, or of two assemblages of complete circuits, cannot produce the continuous rotatory movement in one of those two circuits or assemblages. (See my *Recueil d'Observations Electro-dynamiques*, p. 366.)— You have great reason, as well as M. Nobili, to reproach me with having stated in that passage (of a letter written in great haste), in too general a sense, a fact which is only true of complete circuits, or assemblages of complete circuits, which are *solid;* i. e. *of invariable form in their whole extent.* That it is true in that case, will be easy for you to ascertain; because in every position of two complete circuits, where one tends to impart to the other a motion of continuous rotation, it happens that whenever that motion takes place, the moveable complete circuit supports itself upon the other, and that the motion cannot continue without one of the two circuits having, where they meet each other, a liquid portion that the other can cross. But if I was wrong in that passage of my letter to Mr. Faraday, in not explaining that restriction, by saying " complete solid circuits, and of an invariable form in their whole extent," it was because I thought that the first glimpse of that passage would show that I meant to speak only of that sort of circuit; for the experiment of Mr. Faraday himself (where a magnet turns continually round a vertical conductor) has been known to me for a length of time; and it is evident from my formula, that in that case the continuous motion of rotation must take place, whether the electrical current do or do not cross the magnet, provided the mercury in which it is set up can open to let the magnet pass; in a word, provided the fixed circuit be in a liquid part. I was, moreover, led to think that what I said relative to the impossibility of producing a motion of continuous rotation by their mutual action would be confined to solid complete circuits, as that restriction omitted in my letter to Mr. Faraday was explained most completely in two places of my *Recueil.*

At page 235 of that collection of observations I have thus explained myself: " As soon as I saw, about the end of October 1821, the work of Mr. Faraday, in which he published, a

short time before, his important discovery of the continuous motion of rotation of a voltaic conductor round a magnet and of a magnet round a conductor, and in which he states that he was not enabled by the action of the latter to turn a magnet on its axis, I endeavoured to produce that sort of motion by causing magnets to act in every way I could imagine upon the moveable conductor which I had hitherto made use of in all my experiments, the two extremities of which were placed in the axis of rotation. I soon arrived at this general result, that so long as that circumstance is allowed to exist in a conductor, of which all the parts *are connected invariably together*, the continuous motion of rotation is impossible; and I easily concluded that it was equally so, by the mutual action of a magnet and a complete circuit of *invariable form*, since such a circuit may always be considered as the union of two portions of conductors, of which the extremities are in the same axis of rotation taken at pleasure."

And at page 356, in repeating that it is impossible to produce that sort of motion by employing magnets alone, or *solid* conductors forming complete circuits, I explained in a note at the bottom of the page the expression "*solid conductors*" thus: "It is to be understood by this expression, that all the parts of the portion of that conductor which forms a complete or nearly complete circuit are invariably connected together, and cannot alter their respective situations. When that portion is composed of two or several moveable pieces separately, or is formed entirely or partly of a liquid conductor, the motion of continuous rotation becomes possible."

You perceive, sir, that the limitation which establishes the correctness of what I have advanced, in the case where a rotatory movement becomes impossible, is pointed out in the most express terms in the above note contained in my *Recueil*, immediately preceding my letter to Mr. Faraday, and which was published more than two years ago.

The second observation relates to the remark that you make at page 16 of your paper, in consequence of your having deduced from the expression,

$$\frac{\pi m^2 ii'}{29}\left(\cos\theta' - \cos\theta'' - \cos\theta_1' + \cos\theta_1''\right),$$

which I gave in page 28 of my *Précis de la Théorie des Phénomènes Electro-dynamiques*, to represent the rotatory momentum produced by the action of an electro-dynamic solenoid on a conductor (which action may be in general compared to that produced on the same conductor by a magnet), that, supposing the two extremities of the conductor, and the two poles of the solenoid, or of the magnet, to be at the same time in the axis

of

of rotation, the continuous movement of the axis would take place when one of the poles is between the two extremities of the conductor, and the other pole without the space bounded by these extremities. This result from my formula agrees with that of the experiment made by means of the apparatus shown in Plate I. (fig. 1), although in this apparatus the lower extremity N of the moveable conductor MABN, which is immersed in the mercury of the cup PQ, is not terminated exactly at the axis. This is occasioned, on one hand, by the cosines of the angles θ_1' and θ_1'', relatively to the extremity N, differing but very slightly from the values -1 and $+1$ which these cosines would have if it were precisely in the axis; and on the other hand, by the value of the rotatory momentum, expressed in functions of the angles θ', θ'', θ_1', θ_1'' being applicable to this case, because the various points of the conductor are at much greater distances from the currents from the magnet than the radii of the circumferences described by these currents. But if we could suppose the conductor to penetrate the magnet, and terminate at a point D of the axis situated in its interior, we cannot say precisely what would be the result of such a supposition, which however it would be impossible to realize. For, the points of the portion CD of the moveable conductor being infinitely near to the currents from the magnet, the radii of circumferences described by those currents could no longer be considered as very small, relatively to the distances between each other and the points we are speaking of: thence the expression of the rotatory momentum which has been calculated without reference to the powers of those radii, superior to the third, would cease to give the value of that momentum. Therefore, if in the apparatus we have just described we were to substitute for the magnet an electro-dynamic helix, there would be still a continuous rotatory movement as long as the lower extremity N of the moveable conductor was outside of that helix, as it is outside of the magnet LL' (fig. 1): but if the helix having still for its axis that round which the moveable conductor is made to revolve, the moveable conductor may be disposed as in fig. 2, so that its lower extremity N should be, in like manner as its upper M, precisely in the axis. By making the horizontal portion BC of that conductor pass between the whorls of the helix, it would no longer have a tendency to revolve round the axis of those whorls; because for each of them there would be upon BC a point O, such as the rotatory momentum imparted by the action of the whorl to the portion MABO, in order to make it revolve in one direction, will be destroyed by an equal momentum, and of a contrary sign, resulting from the action of the same whorl to turn the

portion OC in a contrary direction. The opposition of those two actions evidently takes place only from the circumstance that the portion OC of the moveable conductor is in the interior of the helix; whereas the portion MABO is on the outside: but that circumstance cannot take place unless there be points of the moveable conductor at a distance from the two whorls between which it passes, less than that from one whorl to another; and then the value of the rotatory momentum in the function of the angles θ', θ'', θ'_1, θ''_1 is no longer applicable, as it rests upon these two suppositions; 1st, that the distance between two consecutive circular currents is infinitely small; and, 2d, that the distance from the several points of the moveable conductor to those currents is very great, relatively to the radii of the circles which they describe. The case, however, in which the value found for the rotatory momentum no longer exists, is peculiar to the electro-dynamic helices, and cannot be applied to magnets, since the moveable conductor cannot pass between the electric currents to which they owe their properties, and since the radii of the circles described by those currents are of a minuteness corresponding to the order of dimensions of the particles of bodies.

Thus no real difference appears between the action of a magnet and that of an electro-dynamic solenoid. It may be seen that the helix which is substituted for the latter, acts like a magnet, with the exception of that case only where a portion of the moveable conductor passes between its whorls, and extends into the interior of that helix; which circumstance cannot take place with respect to the magnet, of which the circular currents surround each particle. It may at the same time be perceived why the value of the rotatory momentum mentioned above ceases at the same time to express the action of the helix, although it always represents precisely that of the magnets; and how the continuous rotation of the moveable conductor (disposed as in fig. 1) is by no means opposed to the case of equilibrium which I have deduced between the two constants k and n of my formula, the relation

$$2k + n = 1,$$

and which I have proved by the experiment described at pages 311 and 312 of my *Recueil*. In that experiment the equilibrium takes place between the two actions formed by the circular horizontal conductor; the first, in one direction, upon the portion of the moveable conductor which corresponds to the interior of this circular conductor; the second, in a contrary direction, upon the portion of the moveable conductor which is exterior to it. Now, in the apparatus (fig. 1) the latter is quite exterior to the magnet; there is therefore action in

one

Fig. 1.

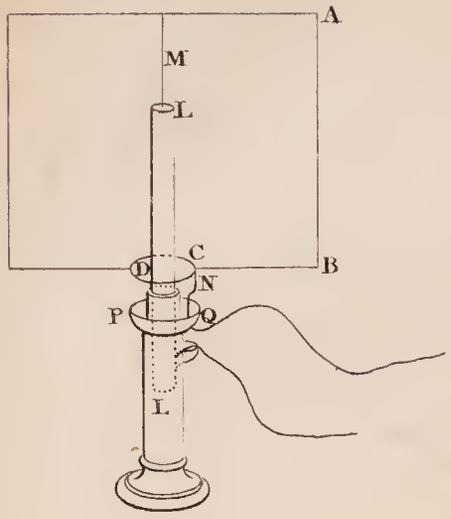


Fig. 2.

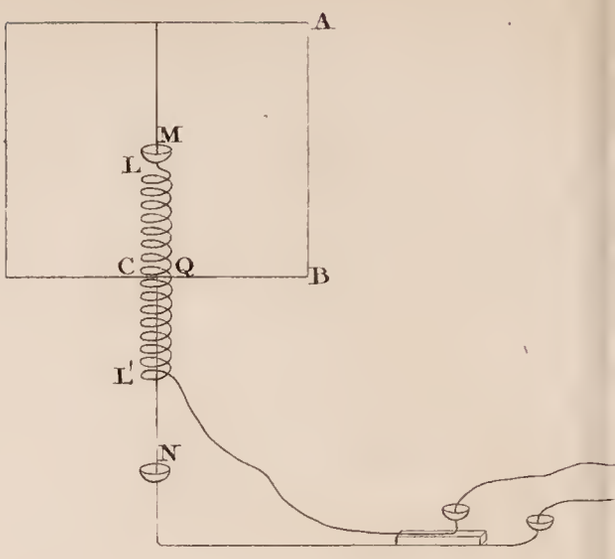


Fig. 4.

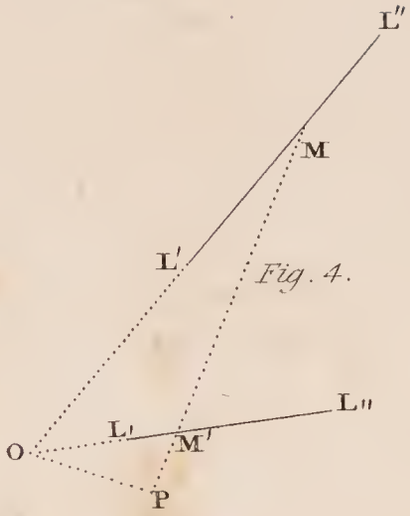


Fig. 3.

Fig. 5.

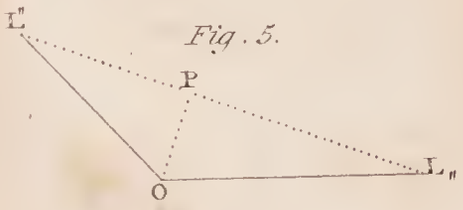
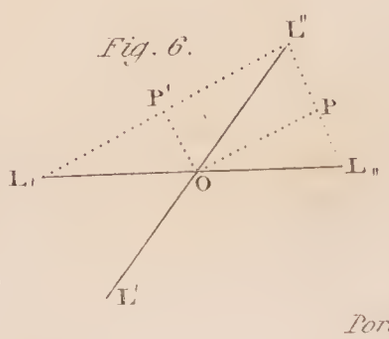


Fig. 6.



Porter Sc.

one direction only, and the movement of continuous rotation is a necessary consequence of it. It is useless to add, that if the actions exerted by the horizontal conductor upon the two portions of the moveable conductor (which I have just mentioned) force it to turn in opposite directions, it is because the current from this last conductor cannot approach that of the horizontal conductor in one of those portions without diverging from it in the other, and *vice versâ*. However, as the manner in which I have established the relation

$$2k + n = 1$$

was not, perhaps, sufficiently rigorous, as I had verified it only on a describing current, either an entire or a semi-circumference, whereas it ought to have been done upon each *element* of the circular horizontal current, I have therefore produced another instrument, by which the same relation between n and k may be obtained in a more simple manner, and the inconvenience which I have just spoken of is avoided; because the experiment which I make with that instrument proves at once that the action of a complete circuit on an element of the electric fluid is always perpendicular to the direction of this element, which is sufficient to demonstrate that $2k + n = 1$, as I shall show in a note which I intend to publish shortly, and where the description of the instrument here presented will be found.

Paris, Aug. 16th, 1825.

LX. *Memoir on a new Electro-dynamic Experiment, on its Application to the Formula representing the mutual Action of the two Elements of Voltaic Conductors, and on some new Results deduced from that Formula.* By M. Ampere.*

THE manner in which I have determined the relation between the two co-efficients of the formula by which I represented the mutual action of the two elements of electric currents, in the memoir which I read before the Academy on the 10th of June 1822, being liable to some difficulties, I have endeavoured to establish this relation in a more simple and direct manner. I succeeded in this very easily by means of an instrument which I shall first describe; I will then present some new results which I have deduced from this formula.

On a stand TT′ Plate I. (fig. 3) in the shape of a table, two

* From the *Annales de Chimie et de Physique*, tom. xxix. p. 381. This memoir was read at the Royal Academy of Sciences of Paris, at the sitting of the 12th of September last.

columns

columns EF, E′F′, are raised, connected with one another by means of two transverse bars LL′, FF′; an axis G H is maintained between these two bars in a vertical position. Its two extremities G, H, terminating in sharp points, enter into two conical apertures, situated one in the lower transverse bar LL′, the other in the end of a screw KZ, borne by the upper transverse bar FF′, and destined to press on the axis GH without forcing it. In C is firmly fixed to this axis an arm QCO, of which the extremity O presents a hinge, in the centre of which is engaged an arc of a circle AA′ formed by a metal wire which always remains in a horizontal position, and the radius of which is the distance from the point O to the axis. This axis is balanced by a counterpoise Q, for the purpose of lessening the friction of the axis GH in the conical apertures in which its extremities are received.

Above the arc AA′ are arranged two small troughs full of mercury, in such a manner that the surface of the mercury rising above the edges shall always come within the arc AA′ in B and B′. These two troughs communicate by means of metallic conductors MN M′N′ with cups P, P′ full of mercury. The cup P and the conductor MN which unites it with the trough M, are fixed to a vertical axis entering into the table so as to be capable of turning freely. The cup P′, to which is fixed the conductor M′N′, is intersected by the same axis, round which it turns as independently as the other. It is isolated from it by means of a glass tube V which covers this axis, and by a glass shield U which separates it from the conductor of the little trough M, in a manner that the conductors MN, M′N′ may be placed at any angle that may be desired.

Two other conductors IR, I′R′ fixed to the table are respectively immersed in the cups P, P′, and make them communicate with cavities R, R′ made in the table and filled with mercury. A third cavity S, also filled with mercury, is between the two others.

The following is the process for using this apparatus: Immerge one of the rheophors (for instance, the positive) into the cavity R, and the negative into the cavity S, which is put in communication with the cavity R′ by a curved conductor of any shape. The current will follow the conductor RI, pass into the cup P, thence into the conductor NM, into the trough M, into the portion BB′ of the arc AA′, in the trough M′, the conductor M′N′, the cup P′, the conductor I′R′, and, at last, from the cavity R′ into the curved conductor which goes into the cavity S in which the negative rheophor is plunged.

According

According to this disposition the whole of the voltaic circuit is composed:

1. Of the arc BB′ and the conductors MN, M′N′.

2. Of a circuit formed by the parts RIP, P′I′R′ of the apparatus, by the curved conductor going from R′ to S, and the pile itself.

This last circuit must act as a complete one, since it is not interrupted except by the thickness of the glass which isolates the two cups P, P′: it will therefore be sufficient to observe its action on the arc BB′ in order to establish by experiment the action of a complete circuit upon an arc in the different positions we may give it.

When, by means of the joint O, the arc AA′ is placed in such a position that its centre is outside the axis G H, this arc begins to move, and slides on the mercury of the little troughs MM′ by the force of the action of the complete curved current, which runs from R′ into S. If on the contrary its centre is in the axis, it remains immoveable: the complete circuit has therefore no action to make it turn round the axis, and that whatever be the size of the part BB′ determined by the opening of the angle of the conductors MN, M′N′. If, therefore, we take two arcs BB′ differing little from each other, as the momentum of rotation is null for either of them, it will be null for their little difference, and therefore for every element of the circumference, the centre of which is in the axis; whence it follows that the direction of action which the complete circuit exercises on the element, passes through this axis, and is thus perpendicular to the element.

When the arc AA′ is situated so that its centre is in the axis, the portions of the conductors MN, M′N′ exercise on the arc BB′ equal and opposite repulsive actions, in such a manner that no effect can result from it; and since there is no motion, we are sure that there is no momentum of rotation produced by the complete circuit.

When the arc AA′ moves in the other situation in which we supposed it first, the actions of the conductors MN and M′N′ are no longer equal. One might be led to believe that the motion is owing only to this difference; but in proportion as we approach or remove the curved conductor running from R′ to S, the movement is increased or diminished; which circumstance leaves no room for doubt that the complete circuit bears a great share in the effect noticed.

If we once establish by this experiment that the action of a complete circuit on an element of the voltaic circuit is always perpendicular to the direction of this element, we may, by a very simple calculation, deduce from it the relation between

tween n and k, which I had before found by another process. It is sufficient for this purpose to decompose the action which each of the elements of the complete circuit exercises on the element in consideration, into two forces; the one perpendicular to this element, and the other which shall have the same direction with itself, and which I shall call the *elementary tangential force:* then to sum up all the elementary tangential forces produced by the complete circuit, and to equal this sum with zero, which is the tangential force due to the whole circuit. Thus then, if we represent by $\mathrm{d}s'$ the element on which it acts, by $\mathrm{d}s$ an element of this same circuit, and otherwise preserve the denominations of the memoir printed in the *Annales de Chimie et de Physique*, tome xx., p. 398, et seq. we shall have for the mutual action of the two elements,

$$-ii'r^{1-n-k}\,\mathrm{d}\,(r^k\,\mathrm{d}'r \quad \text{(page 413)};$$

moreover

$$\cos\beta = -\frac{\mathrm{d}r}{\mathrm{d}s'} \text{ (page 408)},$$

whence

$$\mathrm{d}'r = \frac{\mathrm{d}r}{\mathrm{d}s'}\mathrm{d}s' = -\mathrm{d}s'\cos\beta,$$

which changes the expression of this action into

$$ii'\,\mathrm{d}s'\,r^{1-n-k}\,\mathrm{d}\,(r^k\cos\beta);$$

for $\mathrm{d}s'$ which represents the element on which the complete circuit acts, is constant with respect to the characteristic d.

In order to have the elementary tangential force, we must multiply this value by $\cos\beta$, which gives

$$ii'\,\mathrm{d}s'\,r^{1-n-k}\cos\beta\,\mathrm{d}\,(r^k\cos\beta),$$

which may be put under the form

$$\tfrac{1}{2}ii'\,\mathrm{d}s'\,r^{1-n-2k}\,\mathrm{d}\,(r^k\cos\beta)^2.$$

Integrating by parts, we obtain for the total of the tangential force

$$\tfrac{1}{2}ii'\,\mathrm{d}s'\left\{r^{1-n-2k}(r^k\cos\beta)^2-(1-n-2k)\int r^{-n-2k}(r^k\cos\beta)^2\,\mathrm{d}r\right\};$$

or

$$\tfrac{1}{2}ii'\,\mathrm{d}s'\left\{\frac{\cos^2\beta}{r^{n-1}}-(1-n-2k)\int\frac{\cos^2\beta}{r^n}\,\mathrm{d}r\right\}.$$

As the circuit is closed, r and β will take the same value at the limits; thus the first part

$$\frac{\cos^2\beta}{r^{n-1}}$$

will disappear. But it will not be the same with the second, which cannot be calculated till we have replaced one of the variables r and β by its value in the function of the other drawn

from

from the equations of the circuit, so that we may choose these equations in such a manner that the integral

$$\int \frac{\cos^2 \beta}{r^n} \, \mathrm{d}\, r$$

is not reduced to zero between the limits. In order to remove the total of the tangential force, it is necessary that the coefficient of this integral be null; which gives the relation sought for, $2k + n - 1 = 0$.

In order to form a juster idea of the integral

$$\int \frac{\cos^2 \beta}{r^n} \, \mathrm{d}\, r,$$

we may conceive, round the middle of the element $\mathrm{d}\, s$ taken for a centre, an infinity of spherical surfaces, which divide the complete circuit into infinitely small arcs, so that the two extreme spherical surfaces touch it at the two points of this circuit, which are, one the furthest from, and the other the nearest to, the middle of the element; then we may consider the complete circuit as being composed of two branches terminating at these two points, and both divided into an equal number of infinitely small arcs, so that every arc of one branch corresponds with that of the other branch comprised between the two same consecutive spherical surfaces: for two corresponding arcs we have then the same value of r, and the values of $\mathrm{d}\, r$ are equal, but of contrary signs, for the current cannot go, in withdrawing from the element $\mathrm{d}\, s'$ into one of the branches, without going, in approaching it, into the other. Thence we see why the integral $\int f(r) \, \mathrm{d}\, r$ is always null when it is taken in the whole extent of the complete circuit, since this integral is then composed of elements which are, two by two, of equal value, but of different signs.

It would be the same with $\int f(r) \cos^2 \beta \, \mathrm{d}\, r$, if $\cos^2 \beta$ had the same value for any two corresponding elements; *ex. gr.* if these two elements were always situated symmetrically on the two sides of a plane raised perpendicularly on the middle of $\mathrm{d}\, s'$; but if, on the contrary, in one of the two branches the absolute value of $\cos \beta$ for every element is greater than for its correspondent, $\int f(r) \cos \beta \, \mathrm{d}\, r$ will be composed of two series of terms, one of which will contain only positive terms, and the other negative terms; so that each of the former shall have an absolute value greater or smaller than that of the negative term corresponding with it in the other series. Then this integral can never be null; and in order to make the tangential force comformable to experience, we must have $2k + n - 1 = 0$.

Setting out from this relation between k and n, and naming β' and β'', r' and r'', these values of β and r which correspond with the two extremities of a portion of the voltaic conductor, we find, for the action which it exercises on the element $\mathrm{d}s'$ in the direction of this element

$$\tfrac{1}{2} i i' \,\mathrm{d}s' \left[\frac{\cos^2 \beta''}{r''^{n-1}} - \frac{\cos^2 \beta'}{r'^{n-1}} \right],$$

or rather

$$\tfrac{1}{2} i i' \,\mathrm{d}s' \left[\frac{\cos^2 \beta''}{r''} - \frac{\cos^2 \beta'}{r'} \right],$$

since we know from other experiments that $n = 2$. It is sufficient to change the sign of this expression, which is independent of the form of the portion of the voltaic conductor, and only depends on the situation of its two extremities with respect to the element $\mathrm{d}s'$, in order to have the force with which the same portion of the conductor is drawn in a contrary direction by the element following a right line parallel to the direction of the latter; whence it follows that if this element forms a part of a fixed rectilinear conductor, we shall have the value of the force which the whole conductor exercises, in order to move that portion of which we are speaking, in a direction parallel to this conductor, by integrating between the limits marked by its two extremities the value which we have just found for the tangential force of the element $\mathrm{d}s'$.

If we call a' and a'' the lowered perpendiculars of the two extremities of the portion of the conductor which we consider as moveable, on the rectilinear conductor which we have to calculate the action parallel to its direction, we shall have

$$r'' = \frac{a''}{\sin \beta''}, \quad r' = \frac{a'}{\sin \beta'},$$

$$\mathrm{d}s' = -\frac{\mathrm{d}' r''}{\cos \beta''} = \frac{a'' \,\mathrm{d}\beta''}{\sin^2 \beta''} = -\frac{\mathrm{d}' r'}{\cos \beta'} = \frac{a' \,\mathrm{d}\beta'}{\sin^2 \beta'},$$

and consequently,

$$\frac{\mathrm{d}s'}{r''} = \frac{\mathrm{d}\beta''}{\sin \beta''}, \quad \frac{\mathrm{d}s'}{r'} = \frac{\mathrm{d}\beta'}{\sin \beta'};$$

whence it is easy to conclude that the integral sought for is

$$-\tfrac{1}{2} i i' \int \left[\frac{\cos^2 \beta'' \,\mathrm{d}\beta''}{\sin \beta''} - \frac{\cos^2 \beta' \,\mathrm{d}\beta'}{\sin \beta'} \right]$$

$$= -\tfrac{1}{2} i i' \left[1 \frac{\tan \tfrac{1}{2} \beta''}{\tan \tfrac{1}{2} \beta'} + \cos \beta'' - \cos \beta' + \mathrm{C} \right].$$

We must take this integral between the limits determined by the two extremities of the rectilinear conductor; by calling β_I', β_I'', β_{II}', β_{II}'' the values of β' and of β'' relative to those limits, we have immediately that of the force exercised by the rectilinear conductor, and that last value evidently depends only on the four angles β_I', β_I'', β_{II}', β_{II}''.

When

When we want the value of this force in a case where the rectilinear conductor extends indefinitely in the two directions, we must make $\beta_{I}' = \beta_{I}'' = 0$, and $\beta_{II}' = \beta_{II}'' = \pi$: it seems at first sight that then it becomes null, which would be contrary to experience; but we easily see that the part of the integral in which are the cosines of these four angles, is the only one which vanishes in this case, and that the rest of the integral

$$\tfrac{1}{2}\, ii' \left[l \frac{\operatorname{tang} \tfrac{1}{2}\beta_{I}''}{\operatorname{tang} \tfrac{1}{2}\beta_{I}'} - l \frac{\operatorname{tang} \tfrac{1}{2}\beta_{II}''}{\operatorname{tang} \tfrac{1}{2}\beta_{I}''} \right]$$

$$= \tfrac{1}{2}\, ii' \, l \frac{\operatorname{tang} \tfrac{1}{2}\beta_{I}'' \cot \tfrac{1}{2}\beta_{II}''}{\operatorname{tang} \tfrac{1}{2}\beta_{I}' \cot \tfrac{1}{2}\beta_{I}''},$$

becomes

$$\tfrac{1}{2}\, ii' \, l \frac{\operatorname{tang}^2 \tfrac{1}{2}\beta_{I}''}{\operatorname{tang}^2 \tfrac{1}{2}\beta_{I}'} = ii' \, l \frac{\operatorname{tang} \tfrac{1}{2}\beta_{I}''}{\operatorname{tang} \tfrac{1}{2}\beta_{I}'} = ii' \, l \frac{a''}{a'}.$$

This value shows that the force sought for then only depends on the relation of the two perpendiculars a' and a' lowered on the rectilinear and indefinite conductor of the two extremities of that portion of the conductor on which it acts; that it is also independent of the form of this portion, and only becomes null, as it ought, when the two perpendiculars are equal to themselves.

In order to have the distance of this force from the rectilinear conductor, the direction of which is parallel to its own, we must multiply every one of the elementary forces of which it is composed by its distance from the conductor, and integrate the result with reference to the same limits; we shall thus have the momentum to be divided by the force in order to obtain the distance sought for.

We easily find, after the above values, that the value of the elementary momentum is

$$\tfrac{1}{2}\, ii' \, \mathrm{d}\, s' \, r \sin \beta \, \mathrm{d} \left(\frac{\cos^2 \beta}{r} \right).$$

This value cannot be integrated but by substituting for one of the variables r or β its value in the function of the other, drawn from the equations which determine the form of the moveable portion of the conductor. It becomes very simple when this portion is found on a right line elevated on some point of the rectilinear conductor, which is considered as perpendicularly fixed in its direction, because in taking this point as the origin of s', we have

$$r = - \frac{s}{\cos \beta},$$

because s' is a constant relatively to the differential

$$\mathrm{d} \frac{\cos^2 \beta}{r};$$

the value of the elementary momentum therefore becomes

$$\tfrac{1}{2}\, ii' \, \mathrm{d}\, s' \frac{\sin \beta}{\cos \beta} \, \mathrm{d} \left(\cos^3 \beta \right) = - \tfrac{3}{2}\, ii' \, \mathrm{d}\, s' \sin^2 \beta \cos \beta \, \mathrm{d}\, \beta,$$

the integral of which, between the limits β'' and β' is

$$-\tfrac{1}{2}\, i i'\, \mathrm{d}s' \,[\sin^3 \beta'' - \sin^3 \beta'].$$

Replacing $\mathrm{d}s'$ by the values of this differential found above, and integrating again, we have between the limits determined by the two extremities of the rectilinear conductor,

$$\tfrac{1}{2}\, i i' \,[a'' (\cos \beta_{II}'' - \cos \beta_I'') - a' (\cos \beta_{II}' - \cos \beta_I')].$$

If we suppose that this conductor extends indefinitely in the two directions, we must give to β_I', β_I'', β_{II}', β_{II}'' the values which we have already assigned them in this case, and we shall have $-ii'(a''-a')$ for the value of the momentum of rotation, which will consequently be proportionate to the length $a''-a'$ of the moveable conductor, and will not change, so long as that length remains the same, whatever may be the distances of the extremities of this latter conductor from that which is considered fixed.

It is easy to see that this value is that of the momentum of rotation which the fixed conductor imparts to the portion of another rectilinear conductor, situated on a right line which intersects the direction of the former at right angles, for the purpose of making it turn round the point of intersection of the directions of the two conductors. If we descend from the top of the right angle thus formed, to their intersection, by the direction of the two currents from the perpendiculars upon the four right lines which join, two by two, the extremities of these currents, and if we represent these perpendiculars by p_I', p_I'', p_{II}', p_{II}'', we shall have

$$p_I' = \pm a' \cos \beta_I',\ p_I'' = \pm a'' \cos \beta_I'',\ p_{II}' = \pm a' \cos \beta_I'',$$
$$p_{II}'' = \pm a'' \cos \beta_{II}'',$$

according as the current from the conductor which has been considered as fixed, is approaching or withdrawing from the point where the direction of this conductor meets that of the other; and the value of the momentum of rotation, with which it tends to revolve round this point of the moveable conductor, becomes consequently

$$\pm \tfrac{1}{2}\, i i' \,(p_{II}'' - p_I'' - p_{II}' + p_I'),$$

that is to say, precisely the same as if it were produced by four forces equal to $\frac{1}{2}\, i i'$; of which two would be attractive and in the direction of the right lines which join the extremities of the same name of the two conductors, and the two others repulsive and acting in the direction of the right lines which join the extremities of different names of the same conductors.

If the currents extend to the point of intersection of the directions of the two conductors, three of these four perpendiculars will be null, and the momentum of rotation will be simply proportional to the height of the right-angled triangle of which

which these two conductors will be the sides; so that if it be supposed that their length be increased or diminished in the same proportion, the momentum of rotation will also be increased or diminished in the same proportion.

The result we have just obtained is but a particular case of the general value of the momentum of rotation resulting from the mutual action of two rectilinear conductors $L'L''$, $L_{,}L_{,,}$ (fig. 4), situated in the same plane, in order to make each other revolve round the point of intersection O of their directions. In order to calculate more easily the value of this momentum, which we shall call M, we shall place that of the mutual action of the two elements ds, ds' under this form,

$$\tfrac{1}{2}\, i\, i'\, \frac{ds'}{\cos\beta}\, d\left(\frac{\cos^2\beta}{r}\right),$$

which results immediately from the circumstance that the component of this action, in the direction of the element ds', becomes

$$\tfrac{1}{2}\, i\, i'\, ds'\, d\left(\frac{\cos^2\beta}{r}\right),$$

as we have just seen, in making $k = -\tfrac{1}{2}$ and $n = 2$.

If we take the point of intersection of the directions of the two conductors for the origin of the distances $OM = s$, $OM' = s'$, we shall have $s' \sin\beta$ for the perpendicular OP lowered from this point on the right line which joins the centres of the two elements, and for the value of the elementary momentum of rotation,

$$\frac{d^2 M}{ds\, ds'}\, ds\, ds' = \tfrac{1}{2}\, i\, i'\, s'\, ds'\, \text{tang}\,\beta\, d\left(\frac{\cos^2\beta}{r}\right),$$

whence it is concluded,

$$\frac{dM}{ds'}\, ds' = \tfrac{1}{2}\, i\, i'\, s'\, ds' \left(\frac{\sin\beta\cos\beta}{r} - \int\frac{d\beta}{r}\right).$$

But according to the manner in which the angles have been taken in the formula representing the mutual action of the two elements of voltaic conductors, the angle β is external to the triangle OMM'; and by calling ε the angle MOM' comprised between the directions of the two currents, the third angle OMM', equal to α, will also be so to $\beta - \varepsilon$, which gives

$$r = \frac{s' \sin\varepsilon}{\sin(\beta-\varepsilon)},$$

we have therefore

$$\frac{dM}{ds'}\, ds' = \tfrac{1}{2}\, i\, i'\, \frac{ds'}{\sin\varepsilon}\,[\cos\beta \sin\beta \sin(\beta-\varepsilon) + \cos(\beta-\varepsilon) + C].$$

Replacing in this value $\cos(\beta-\varepsilon)$ by

$$\cos^2\beta \cos(\beta-\varepsilon) + \sin^2\beta \cos(\beta-\varepsilon),$$

it will easily be seen that it is reduced to

$$\frac{dM}{ds'}\, ds' = \tfrac{1}{2}\, i\, i'\, \frac{ds}{\sin\varepsilon}\,[\cos\varepsilon \cos\beta + \sin^2\beta \cos(\beta-\varepsilon) + C],$$

which

which we must take between the limits β' and β''. We have also the difference of the two functions of the same form, one of β'', the other of β', which must be again integrated, in order to obtain the rotation sought for: it is enough to make this second integration upon one of these two quantities: let a'' then be the distance OL$''$ which answers to β'', we shall have

$$s' = \frac{a'' \sin(\beta'' - \varepsilon)}{\sin \beta''} = a'' \cos \varepsilon - a'' \sin \varepsilon \cot \beta'', \; \mathrm{d}s' = \frac{a'' \sin \varepsilon \, \mathrm{d}\beta''}{\sin^2 \beta''};$$

and the quantity which we shall wish to integrate first will be

$$\tfrac{1}{2} a'' i i' \left\{ \frac{\cos \varepsilon \cos \beta'' \, \mathrm{d}\beta''}{\sin^2 \beta''} + \cos(\beta'' - \varepsilon) \, \mathrm{d}\beta \right\},$$

the integral of which taken between the limits $\beta''_{\prime}$ and $\beta''_{\prime\prime}$ is

$$\tfrac{1}{2} a'' i i' \left\{ \sin(\beta''_{\prime\prime} - \varepsilon) - \sin(\beta''_{\prime} - \varepsilon) - \frac{\cos \varepsilon}{\sin \beta''_{\prime\prime}} + \frac{\cos \varepsilon}{\sin \beta''_{\prime}} \right\}.$$

Designating by $p''_{\prime\prime}$ and $p''_{\prime}$, the perpendiculars lowered from the point O on the distances $\mathrm{L}''\mathrm{L}_{\prime\prime} = r''_{\prime\prime}$, $\mathrm{L}''\mathrm{L}_{\prime} = r''_{\prime}$, we have evidently

$$a'' \sin(\beta''_{\prime\prime} - \varepsilon) = p''_{\prime\prime}, \; a'' \sin \beta''_{\prime} = p''_{\prime}, \; \frac{a''}{\sin \beta''_{\prime\prime}} = \frac{r''_{\prime\prime}}{\sin \varepsilon}, \; \frac{a''}{\sin \beta''_{\prime}} = \frac{r''_{\prime}}{\sin \varepsilon},$$

and the preceding integral becomes

$$\tfrac{1}{2} i i' \left[p''_{\prime\prime} - p''_{\prime} - (r''_{\prime\prime} - r''_{\prime}) \cot \varepsilon \right].$$

If we notice that by designating the distance OL$'$ by a', we have also

$$s' = \frac{a' \sin(\beta' - \varepsilon)}{\sin \beta'} = a' \cos \varepsilon - a' \sin \varepsilon \cot \beta', \; \mathrm{d}s' = \frac{a' \sin \varepsilon}{\sin^2 \beta'},$$

we easily see that the integral of the other quantity is formed by that which we have just obtained, changing $p''_{\prime\prime}$, $p''_{\prime}$, $r''_{\prime\prime}$, $r''_{\prime}$ into $p'_{\prime\prime}$, $p'_{\prime}$, $r'_{\prime\prime}$, $r'_{\prime}$, which gives for the value of the momentum of rotation which is the difference of the two integrals

$$\tfrac{1}{2} i i' \left[p''_{\prime\prime} - p'_{\prime\prime} - p'_{\prime\prime} + p'_{\prime} - (r''_{\prime\prime} - r'_{\prime\prime} - r'_{\prime\prime} + r'_{\prime}) \cot \varepsilon \right].$$

That value is reduced to what we have found above, in the case where the angle ε is right, because then $\cot \varepsilon = 0$.

If we suppose that two currents proceed from point O, and that their lengths OL$''$, OL$_{\prime\prime}$ (fig. 5) are respectively represented by a and b, the perpendicular O P by p, and the distance $\mathrm{L}''\mathrm{L}_{\prime\prime}$ by r, we shall have

$$\tfrac{1}{2} i i' \left[p + (a + b - r) \cot \varepsilon \right],$$

for the value which, in this case, the momentum of rotation takes.

The quantity $a + b - r$, the excess of the sum of two sides of a triangle on the third, is always positive; whence it follows that the momentum of rotation is greater than the value $\frac{1}{2} i i' p$ which it takes when the angle ε of the two conductors is a right

right one, whilst cot ε is positive, *i. e.* whilst this angle is acute; but it becomes smaller when the same angle is obtuse, because then cot ε is negative. Moreover, it is evident that its value becomes so much greater as the angle ε is smaller, and that it increases *ad infinitum* like cot ε in proportion as cot ε approaches zero; but it will be well to show that it always remains positive, however near this angle be to two right lines.

For that, it is sufficient to observe that by calling α the angle of the triangle $OL''L_{''}$ comprised between the sides a and r, and β that which is between the sides b and r, we have cot $\varepsilon = -\cot(\alpha+\beta)$, $p = a\sin\alpha = b\sin\beta$, $r = a\cos\alpha + b\cos\beta$, and consequently

$$a + b - r = a(1-\cos\alpha) + b(1-\cos\beta)$$
$$= p\,\mathrm{tang}\,\tfrac{1}{2}\alpha + p\,\mathrm{tang}\,\tfrac{1}{2}\beta,$$

and

$$\tfrac{1}{2}ii'\left[p + (a+b-r)\cot\varepsilon\right] = \tfrac{1}{2}ii'p\left(1 - \frac{\mathrm{tang}\,\frac{1}{2}\alpha + \mathrm{tang}\,\frac{1}{2}\beta}{\mathrm{tang}\,(\alpha+\beta)}\right),$$

a value which always remains positive, however small the angles α and β, since tang $(\alpha+\beta)$, for inferior angles at $\frac{\pi}{4}$, is always larger than tang α + tang β, and of course more so than tang $\frac{1}{2}\alpha$ + tang $\frac{1}{2}\beta$. This value evidently tends towards the limit $\frac{1}{4}ii'p$ in proportion as the angles α and β approach zero: it vanishes with p when these angles become null.

Departing from this expression at the momentum of rotation resulting from the mutual action of the two rectilinear conductors situated in the same plane, round the point of intersection of their direction, and of the general fact proved again by the experiment described in the beginning of this Memoir, of the nullity of action of a conductor bent in an arc upon a portion of the circuit, the two extremities of which are in the perpendicular raised in the centre of this arc upon the plane on which it is described, I have devised an instrument founded on the same principle as that which I presented about two years ago to the Academy of Sciences, and described in my *Recueil d'Observations Electro-dynamiques*, p. 224, &c. Its object is also to determine by the number of oscillations of a moveable conductor, the value of the action which a fixed conductor exercises upon it; but which has not the inconvenience that was found in the first, of giving the experimental measure of this action in a case in which it cannot be determined, by the aid of my formula, but by calculations of the most complicated kind. I intend soon to publish the description of this instrument.

When the point of intersection of the conductors OL'', $L_{'}L_{''}$

$L_{/}\,L_{//}$ (fig. 6) is found at one of the extremities of the first and in the middle of the second, we obtain the momentum of rotation resulting from the mutual action of these two conductors, with the addition of those referring to each of the angles $L_{/}OL''$, $L_{//}OL''$, of which the two cotangents are equal and of a contrary sign; so that in marking the distances $L_{//}L''$ and $L_{/}L''$ by r and r', and the perpendiculars OP, OP' by p and p', we have for that momentum

$$\tfrac{1}{2}\,ii'\,[p + p' + (r' - r)\cot\varepsilon].$$

Let us moreover suppose that the length $OL'' = a$ of the conductor which has one of its extremities in O is equal to half $OL_{/}$ or $OL_{//}$ of the other, and let us call θ the half POL'' or $POL_{//}$ of the angle $L_{//}OL'' = \varepsilon$, we shall find

$$p = a\cos\theta,\ p' = a\sin\theta,\ r = 2\,a\sin\theta,\ r' = 2\,a\cos\theta,$$

$$\cot\varepsilon = \frac{1-\operatorname{tang}^2\theta}{2\operatorname{tang}\theta} = -\frac{1-\cot^2\theta}{2\cot\theta}:$$

the value of the momentum of rotation therefore is

$$\tfrac{1}{2}\,aii'\left\{\cos\theta - \sin\theta\frac{1-\operatorname{tang}^2\theta}{\operatorname{tang}\theta} + \sin\theta - \cos\theta\frac{1-\cot^2\theta}{\cot\theta}\right\},$$

or

$$\tfrac{1}{2}\,aii'\,[\cos\theta\operatorname{tang}^2\theta + \sin\theta\cot^2\theta] = \tfrac{1}{2}\,aii'\,[\sin\theta\operatorname{tang}\theta + \cos\theta\cot\theta].$$

It is sufficient to double the expression, suppressing the denominator 2, for that produced by the action of the two conductors $L'L''$, $L_{/}L_{//}$ of the same length, and the centres of which are at the point O round which one of them is supposed to be moveable.

In the instrument of which I have just spoken, there are two rectilinear conductors of equal length, moveable round their centres; from each of these centres, and sufficiently apart that there may not be between the conductors a sensible mutual action, project two other rectilinear conductors half the length of the others; these are fixed, and form between themselves an angle that may be varied at will: the same electric current runs through the six conductors; so that in every one of the fixed ones, and in that part of the corresponding moveable conductor nearest to it, its current is in a contrary direction, in order that the latter may keep in a steady equilibrium in the perpendicular direction on the right line, which divides into two equal parts the angle of the two fixed conductors whose action it experiences. As it is this latter angle which is given immediately above the graduated arc attached to one of these fixed conductors, it is desirable to introduce its half, which we represent by η, instead of θ in the expression of the momentum of rotation

$$M = \tfrac{1}{2}\,a\,i\,i'\,(\sin\theta\operatorname{tang}\theta + \cos\theta\cot\theta)$$

which

which every fixed conductor impresses on the moveable conductor on which it acts, if we observe that

$$\sin^3\theta + \cos^3\theta = (\sin\theta + \cos\theta)(1 - \sin\theta\cos\theta),$$

and that $$\theta = \tfrac{1}{2}\varepsilon = \tfrac{1}{2}\left(\frac{\pi}{2} - \eta\right)$$

gives

$$\sin\theta\cos\theta = \tfrac{1}{2}\sin\varepsilon = \tfrac{1}{2}\cos\eta \text{ and } \sin\theta + \cos\theta = \sqrt{2}\cos\tfrac{1}{2}\eta,$$

we shall have $$M = \frac{1}{\sqrt{2}}\, a\, i\, i'\cos\tfrac{1}{2}\eta\left(\frac{2}{\cos\eta} - 1\right).$$

When the moveable conductor is displaced ever so little from the situation of equilibrium, the angle θ becomes $\theta + d\theta$ with reference to one of the fixed conductors, and $\theta - d\theta$ with reference to the other; so that the difference of the two momenta, which was null in that situation, becomes, after being displaced,

$$2\frac{dM}{d\theta}\,d\theta = -a\,i\,i'(\cos\theta - \sin\theta)\left(\frac{1}{\sin^2\theta\cos^2\theta} + \frac{1}{\sin\theta\cos\theta} + 1\right)d\theta.$$

This value is always negative when we take, as we suppose it here, the angle ε double of θ on that side where this angle is acute, and consequently after the opposite direction of the electric currents in the two sides of this angle, which requires that the momentum M should tend to increase it, the momentum $2\frac{dM}{d\theta}\,d\theta$ will tend to diminish it on the side where $d\theta$ is positive, and to increase it on the side where the same differential is negative; *i. e.* to bring back the moveable conductor to the position of equilibrium, which might besides easily be seen *à priori.*

If we introduce in the value just obtained the angle η instead of θ, we find

$$2\frac{dM}{d\theta}\,d\theta = -a\,i\,i'\sqrt{2}\sin\tfrac{1}{2}\eta\left(\frac{4}{\cos^2\eta} + \frac{2}{\cos\eta} + 1\right)d\theta.$$

This momentum must be measured, either by the torsion of a thread, or by the number of oscillations made by the two moveable conductors, in a given time, by means of observations made simultaneously * upon these conductors, when we wish to verify the results deduced from my formula, comparing them with those of experience.

I have also devised another apparatus which may serve for the same verifications, by calculating, and afterwards mea-

* By comparing among themselves measures determined by successive operations, we also completely avoid the inaccuracy produced by the variations of the energy of the pile, which necessarily alter the results deduced from experiment. We may also measure in a direct manner the momentum M by the torsion of a thread.

suring, the angles which a rectilinear conductor, moveable round its centre, forms, in the situation of equilibrium, with two fixed conductors, the directions of which are through this centre, and one of which is traversed several times by the electric current, which traverses the other but once. It is thus we obtain the relation which ought to subsist between the momentum of rotation; and we verify it easily if the value of these momenta, calculated after my formula, agrees with experience.

If the conductor, the length of which has been designated by b, were to extend *ad infinitum*, the other, whose length is always $2a$ and its middle situated on the direction of the first, we should have

$$p = p' = a \sin \varepsilon,\ r' - r = 2a \cos \varepsilon,$$

and the value of the momentum of rotation

$$\tfrac{1}{2} a\, i\, i' \,[p + p' + (r' - r) \cot \varepsilon]$$

would become $$a\, i\, i' \left[\sin \varepsilon + \frac{\cos^2 \varepsilon}{\sin \varepsilon}\right] = \frac{a\, i\, i'}{\sin \varepsilon}.$$

When the fixed conductor is indefinite in its two directions we must double this value, and we have

$$\frac{2a\, i\, i'}{\sin \varepsilon}$$

for the momentum of rotation which it gives to the moveable conductor $2a$. This momentum is therefore reciprocally proportional to the sine of the angle ε formed by the directions of the two conductors.

The expression $\tfrac{1}{2}\, i\, i'\, \mathrm{d}\, s' \left(\frac{\sin \beta \cos \beta}{r} - \int \frac{\mathrm{d}\, \beta}{r}\right)$,

which I gave (in 1822) in my *Recueil*, page 331, for the value of the component perpendicular to the element $\mathrm{d}\, s'$, may serve to calculate very easily the mutual action of two parallel conductors; for, by calling a the distance of these two conductors, we have first $$r = \frac{a}{\sin \beta},$$

which gives, for the value of the preceding integral between the limits β', β'',

$$\tfrac{1}{2}\, i\, i' \frac{\mathrm{d}\, s'}{a} (\cos \beta'' \sin^2 \beta'' + \cos \beta'' - \cos \beta' \sin^2 \beta' - \cos \beta'),$$

then at each limit, presenting the values of s by b' and b'',

$$s' = b'' - a \cot \beta'' = b' - a \cot \beta',\ \mathrm{d}\, s' = \frac{a\, \mathrm{d}\, \beta''}{\sin^2 \beta''} = \frac{a\, \mathrm{d}\, \beta''}{\sin^2 \beta'}.$$

By substituting these values and integrating again between the

the limits β_I', β_{II}' and β_I'', β_{II}'', we have, for the value of the force sought for,

$$\tfrac{1}{2}\, i\, i' \left(\sin \beta_{II}'' - \sin \beta_I'' - \sin \beta_{II}' + \sin \beta_I' - \frac{1}{\sin \beta_{II}''} + \frac{1}{\sin \beta_I''} + \frac{1}{\sin \beta_{II}'} - \frac{1}{\sin \beta_I'}\right),$$

or

$$\tfrac{1}{2}\, i\, i' \left(\frac{a}{r_{II}''} - \frac{a}{r_I''} - \frac{a}{r_{II}'} + \frac{a}{r_I'} + \frac{r_I'' + r_{II}' - r_{II}'' - r_I'}{a}\right).$$

If the two conductors are of the same length, and perpendicular to the right lines which join by pairs their extremities on the same side, we have

$$r_I' = r_{II}'' = a, \text{ and } r_{II}' = r_I'' = c.$$

Calling c the diagonal line of the rectangle formed by these two right lines and the direction of the two currents, the foregoing expression then becomes

$$i\, i' \left(\frac{c}{a} - \frac{a}{c}\right) = \frac{i\, i'\, l^2}{a\, c}.$$

Calling l the length of the conductors, and this rectangle becoming a square, we have $\frac{i\, i'}{\sqrt{2}}$ for the value of the force

finally, if we suppose one of the conductors indefinite in the two directions, and that l be the length of the other, the terms in which r_I', r_{II}', r_I'', r_{II}'' are in the denominator, will disappear, we shall have

$$r_{II}' + r_I'' - r_{II}'' - r_I' = 2\, l,$$

and the expression of the force will become

$$\frac{i\, i'\, l}{a},$$

which is reduced to $i\, i'$ when the length l is equal to the distance a.

[To be continued.]

LXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE meetings of this Society for the session 1825–6 were resumed on the 17th instant; when the following papers were read:—On the changes that have taken place in some ancient alloys of copper, by John Davy, M.D. F.R.S.—Observations on the apparent positions and distances of 468 double and triple fixed stars, made at the observatory at

Passy, near Paris, in the summer of 1825, by James South, F.R.S.

Nov. 24.—A paper was read On the comparison and adjustment of the standards of the new weights and measures, by Capt. H. Kater, F.R.S.

LINNÆAN SOCIETY.

Nov. 1 and 15.—The following paper was read: Observations on the unimpregnated vegetable *ovulum*, and on the nature of the female flower in *Coniferæ* and *Cycadeæ*, by Rob. Brown, Esq. F.R.S. F.L.S. &c. &c.

ASTRONOMICAL SOCIETY OF LONDON.

Nov. 11.—The Society resumed its sittings this evening, and the President took the opportunity of calling the attention of the Members to the remarkable circumstance of the appearance of no less than *four* comets during the recess, an occurrence unparalleled in the history of Astronomy. The *first* of these (he observed) was discovered by M. Gambart, at Marseilles, on May 19, in the head of *Cassiopea*. The *second* by M. Valz, at Nismes, on July 13, near χ *Tauri*. The *third* by M. Pons, at Florence, on Aug. 9, in *Auriga*. The *fourth* (which was the most interesting and important of the whole, since it had been the object of solicitude at every observatory, and was anxiously expected and looked after by every Astronomer) was discovered about July or August last. The President remarked that this last comet (which is better known by the name of the *comet of Encke*) has now made 13 revolutions within the last 40 years: six of which have been regularly observed by Astronomers. It was first seen in 1786; afterwards in 1795, 1805, 1819, 1822, and in the present year. It makes a complete revolution in about 1207 days, or about $3\frac{1}{2}$ years.

A paper was read On the latitude of the Royal Observatory of Greenwich, by the Astronomer Royal. The co-latitude of this observatory, as computed from Dr. Bradley's observations under the direction of Dr. Maskelyne, is $38^\circ\ 31'\ 22'',0$; a determination which is subject to the sum or the difference of two separate errors: one, in determining the zenith distance of γ *Draconis*; the other, in the measure of the distance of that star from the pole.

After the new mural circle was erected in 1812, another attempt was made to determine this important element. The result was $38^\circ\ 31'\ 21'',5$; a result, however, in which it was thought probable that an error of half a second might exist.

In

In the year 1822 a new method of observing was introduced at Greenwich, by means of the *reflected* images of stars from an artificial horizon. To apply this to the determination of the element in question, by comparing two catalogues, one formed by direct vision, the other by reflection, that co-latitude being assumed to be the true one, which made the sum of the small positive and negative differences equal to zero, and that was found to be 38° 31′ 21″, differing by *one second* from the determination furnished by Bradley's observations. This result, however, may involve an error of from a quarter to half a second, which subsequent observations may diminish.

The same paper includes some remarks on observations upon the pole-star, and an interesting circumstance, which is this:—The undulation to which a mass of mercury is liable, even with the greatest care, is, in itself considered, unfavourable to the exact bisection of an image; but a circumstance occurs in the formation of the image in the telescope, which in some measure compensates the inconvenience. The vibrations of the mercury in a longitudinal trough occasion an elongated image of the star in the direction of the wire, appearing like a succession of stars, which become smaller and smaller as they recede from the central undefined mass, exhibiting an appearance like beads threaded on the wire, which is extremely favourable to bisection.

The elements of one of the comets above mentioned were announced to the Society as computed by Mr. Taylor, sen., and Mr. Taylor, jun., of the Royal Observatory, and M. Capreci, of Naples. They are respectively, as below,

	Taylor, sen.	Taylor, jun.	Capreci.
Passage of perihelion	Dec. 10^{d}·9338 Greenwich.M.T.	Dec. 10^{d}·4559 Greenwich.M.T.	Dec. 8^{d}·895 Naples. M.T.
Longitude of ditto .	318° 3′ 57″	319° 10′ 26″	317° 24′ 40″
Longitude of ☊ . .	35 46 58	35 45 36	35 19 50
Inclination of orbit .	33 20 40	33 30 42	32 44 20
Perihelion distance .	1·22951	1·24633	1·20808
Motion	Retrograde. From 3 observ.	Retrograde. From 3 observ.	Retrograde. From 4 observ.

A letter was read from Mr. R. Comfield, a member of the Society, to Dr. Gregory, describing an appearance noticed by him with a Gregorian reflector, power 350, and by Mr. J. Wallis, the lecturer on astronomy, with a Newtonian telescope, power 160, in reference to the occultation of *Saturn* on Oct. 30th. To each of them that part of the ring of *Saturn* which last *emerged* from the moon's dark limb (neither of them

them could observe the immersion) was rendered sensibly more obtuse, and at the instant after separation approximating to a rectilinear boundary. At the emergence of the eastern limb of the *globe* of *Saturn* a similar effect was observed by Mr. Comfield, but not by Mr. Wallis.

A paper was next read On the determination of latitudes by observations of azimuths and altitudes alone, by M. Littrow, Assoc. Ast. Soc. This paper includes the consideration of four cases. In the 1st, the latitude is computed from the observed azimuth and altitude. In the 2d, two observed altitudes are taken, and the two instrumental azimuths at the same respective moments; and the latitude is found from the corrected altitudes, and the *difference* of the azimuths, with the addition of an *approximate* latitude. In the 3d case, three observed latitudes, and three corresponding azimuths, or two azimuthal differences, are required; and the latitude is thence determined. In a 4th case, the problem is solved by means of a watch instead of an azimuth circle; there are supposed given, the time of culmination only within half or three-quarters of an hour, three altitudes taken within that distance of the meridian, and their intervals in time; to find the true latitude. The solutions to all the four cases are exceedingly simple, and the resulting formulæ admit of the utmost facility of application.

Lastly, there was exhibited to the Society a model of one of the large reflecting telescopes made by Mr. John Ramage, of Aberdeen, and of the stands, frame, and mechanism for facilitating its motions and adjustments. The reading of a descriptive paper, by Mr. Ramage, was also commenced; but its termination was postponed until the December meeting.

HORTICULTURAL SOCIETY.

Aug. 16.—The large silver medal was presented to Mr. William Greenshields, a Corresponding Member of the Society, gardener to Richard Benyon de Beauvoir, Esq. F.H.S., for a communication on the cultivation of the pine-apple, which is printed in the Transactions of the Society.—The following paper was read: On the management of *Amaryllis vittata* in the open air, by Mr. John Brown, gardener to Chandos Leigh, Esq. F.H.S., at Stoneleigh Abbey.

Oct. 4.—The following paper was read: On the cultivation of the large varieties of French melons, by the Rev. J. Le Sene.

Oct. 18.—The following papers were read: On the cultivation of the *Passiflora quadrangularis*, by Mr. John Mitcheson.—On the means of obtaining an abundant second crop of melons, by Mr. Charles Harrison, F.H.S.

Nov. 1.—The following paper was read: An account of an easy and successful method of destroying wasps, by Mr. Charles Harrison, F.H.S.

SOCIETY OF PHYSICIANS OF THE UNITED KINGDOM.

At a meeting of the Society of Physicians of the United Kingdom, held November the 2d, the following officers were elected for the ensuing year: —*President*, Dr. Birkbeck; *Treasurer*, Dr. Clutterbuck; *Secretary*, Dr. Shearman.

Communications, whether from members or others, addressed to the Secretary, No. 30, Northampton-square, will be submitted to the consideration of the Society, and the most interesting and important of them selected for publication, as soon as sufficient materials shall be collected to form a volume.

LXII. *Intelligence and Miscellaneous Articles.*

NEW DEMONSTRATION OF A PROBLEM IN TRIGONOMETRY.

To the Editor of the Philosophical Magazine and Journal.

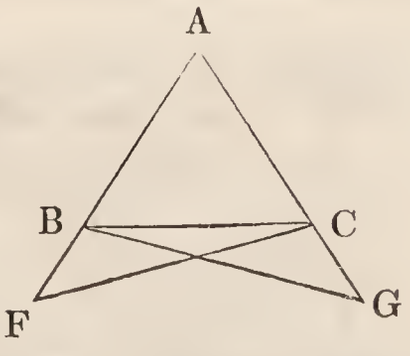
Sir,

SHOULD the following new and concise demonstration of a well-known proposition be deemed worthy a place in the Philosophical Magazine, its insertion will oblige

Your humble servant,

Wisbech. ISAAC NEWTON.

"The angles at the base of an isosceles triangle are equal to each other."



Let ABC be the isosceles triangle, whose equal sides are AB, AC; then will $\prec B = \prec C$. For in AB, AC, take the points F and G equidistant from A, and draw the right lines CF, BG; then (Euc. iv. 1.) it is plain that $\prec F = \prec G$; and for the same reason the $\prec F$ will *always* = the $\prec G$ as long as F and G, the extremities of CF and BG, are equidistant from A. Therefore if F and G be made to fall on B and C, CF and BG will each coincide with BC; and the angles F and G coincide with B and C respectively: but F and G are *always* equal angles, therefore their coincident angles B and C are also equal. Q. E. D.

P.S. The above, as may be perceived, requires not the aid of any subsequent proposition in Euclid.

ON

ON MR. NEWTON'S IMPROVED CROSS FOR LAND-SURVEYING.

To the Editor of the Philosophical Magazine and Journal.

Sir,

The last Number of the Philosophical Magazine contains some observations on Mr. Newton's improved cross for land-surveyors.

The points on the diagonal of a field where the true perpendiculars fall are, I admit, as readily found by means of the old cross as by means of the new one; but the *measuring* of these or any other straight lines is certainly more accurately and more expeditiously accomplished by the latter than the former. "Leander's" calculations, most probably through mere inadvertence, are the very reverse of what they ought to be. Instead of *dividing* the fraction $\frac{1}{5}$ by the numbers 2, 3, 4, &c. he ought to have *multiplied* by these numbers; which would have given, instead of $\frac{1}{10}$, $\frac{1}{15}$, $\frac{1}{20}$, &c. the numbers $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, &c. errors increasing in proportion to the length of the lines to be measured. Now if this new instrument will enable the surveyor without any additional trouble to correct or avoid these errors, there is doubtless an advantage attending the use of it, whatever may be the custom of the surveyor in "reading off" his lines, &c.—"Leander's" method of measuring an inaccessible line appears to me very ingenious: at the same time I think that when a "pond" only intercepts the points *a* and *d*, the line *da* may be measured more readily as follows:—On the same side of *da* take two equal perpendiculars *a* B, *d* C to *da*, so shall the distance from B to C be equal to *da* as was required.

I am, sir, &c.

Wisbech, Nov. 1825. Y. Z.

NOTICE ON MR. TREDGOLD'S CORRECTION OF DR. URE'S CALCULATION OF THE LATENT HEAT OF VAPOURS.

To the Editor of the Philosophical Magazine and Journal.

Sir,

At page 277 of your last Number, Mr. Tredgold points out "a considerable error in the mode of calculating the results of the experiments" of Dr. Ure on the latent heat of vapours, which, he says, "has not been noticed." Now, if your readers will turn to Mr. Herapath's paper in the Annals of Philosophy for December 1821, pp. 450 and 458, they will find the very number 888° for aqueous vapour Mr. T. gives, with Dr. Ure's errors, and the cause of them, particularly pointed out. In Phil. Mag. for Oct. 1822, p. 299, they will also find these same mistakes animadverted upon in a paper signed D.

I am, sir, yours, &c.

CANDOUR.

NEW SURVEY OF THE ZETLAND ISLANDS.

An accurate chart of the Zetland Islands has long been a desideratum in British hydrography. Authorized surveys of them have, it is true, been made; but of these some are almost obsolete, and all are more or less partial or defective: and to errors of this nature, perhaps as much as to any other cause, are to be ascribed many of the disastrous shipwrecks of which that remote country has too often been the melancholy scene.

It is not a little surprising, that while the most extended, expensive, and minute surveys have been executed, by order of the English government, of many distant regions of the globe, the nautical geography of the northern extremity of the British Islands should have been so long suffered to remain in obscurity. Charts are to maritime, what roads are to inland, commerce; and we duly appretiate the laudable and fostering care which our statesmen have evinced to facilitate its extension and stability.

The Zetland Islands have too long been the bugbear, the Scylla and Charybdis of Northern mariners: hence commerce has been repelled from them, and one grand source of their improvement and prosperity injudiciously obstructed. Besides, they might afford a secure refuge and resting-place, not only to vessels trading in the North Sea, but also to others forced by boisterous weather and unavoidable accidents into their latitude. And when, superadded to these circumstances, are considered the barbarous and iron-bound nature of the coast, and the dangerous rapidity and variety of the currents, it cannot but be highly gratifying to learn that this important chasm in our maritime knowledge is in progress of being filled up.

For this purpose the Admiralty, in the month of May this year, sent to Zetland their surveyor, Mr. Thomas, an officer whose ability, experience, and indefatigable zeal are so conspicuous, and who has more particularly displayed his dexterity and talent in his surveys of the two metropolitan rivers of England and Scotland, and their adjacent coasts; and we trust that no delay or impediment will now occur to a work so very desirable, and which will reflect so much honour on the enlightened liberality and humanity of our Admiralty, and on the skill and activity of its surveyor.

The coast of Zetland is every where bold and prominent, and intersected with numerous and excellent harbours, of which the headlands are the sublime and natural beacons; and there are few situations in which the seaman can be placed where the confident guidance of an accurate chart might be of such

paramount utility, and few where the want of it might be so perilous and fatal. Such a chart of Zetland would be a permanent one; unlike in this respect to many, regarding other parts of Great Britain, which require to be frequently modified to suit the changes produced by the action of the waves in the formation and dissolution of sand-banks, and where even the best charts can be too often of little other use, from the scarcity of harbours, than to present more distinctly to the unfortunate mariner the locality of his inevitable and impending shipwreck. (Y.)—*Annals of Philosophy.*

ON THE DISCOVERY OF THE ANOPLOTHERIUM COMMUNE IN THE ISLE OF WIGHT. BY PROFESSOR BUCKLAND.

Since the publication of Mr. Webster's excellent Memoirs on the Geology of the Isle of Wight, and the coasts adjacent to it, no doubt has existed as to the identity of the fresh-water formations that occur so extensively in that island with those described by Cuvier and Brongniart in the vicinity of Paris; and this conclusion has rested on the similarity of the remains of fresh-water molluscæ and vegetables which these formations respectively contain, and on a correspondence in their substance, and their relative position to other strata of marine origin, quite sufficient to establish the contemporaneous deposition of these remarkable strata at the bottom of ancient fresh-water lakes in the districts which are geologically distinguished by the appellation of the basin of Hampshire and the basin of Paris.

There was still, however, a further point on which evidence appeared desirable, inasmuch as the remains of the genus *Anoplotherium* and other large lacustrine quadrupeds which occur in the basin of Paris, had not been ascertained to exist in England. This desideratum I have long felt anxious to supply; and in a rapid excursion to the west of the Isle of Wight two years ago, I sought for the bones of these animals in the cliffs of Headon Hill and Totland Bay, and some adjacent quarries of the interior, without finding any thing more than a small fragment too indistinct to be considered decisive of a point to which no other evidence had yet been adduced. But in the month of November last, whilst occupied in looking over the cabinets of Mr. Thomas Allan, of Edinburgh, I discovered a tooth, which he informed me he had himself collected several years ago in the Isle of Wight, in the quarries of Binstead, near Ride, and which immediately struck me as belonging to one of the animals I had been so long in search of; and on my subsequently showing it to Mr. Pentland (who is accurately versed in all the details of the fossil quadrupeds of the

Paris

Paris basin), he at once pronounced it to be a molar tooth of the lower jaw of the *Anoplotherium commune*.

As the evidence of this tooth's having been found in the quarries of fresh-water limestone at Binstead (I believe the lower fresh-water) rests on such accurate authority as that of Mr. Allan, we may consider this important and almost only deficient link in the chain of evidence that unites the English fresh-water formations with those of France to be now supplied, and hope that this discovery will stimulate others whose local position affords them opportunity, to persevere in the attempt to collect further traces of the remains of this remarkable class of extinct quadrupeds in the fresh-water strata of the Isle of Wight.—*Annals of Philosophy.*

FORMATION OF ORES BY THE ACTION OF THE ATMOSPHERE AND OF VOLCANIC HEAT.

In a late Number of the Phil. Mag. we mentioned the formation of brown hæmatite by the action of water on cast-iron pipes. We have now to enumerate, from the same source, some other facts of a like description.

Nöggerath, in the third volume of his work entitled *Das Gebirge in Rheinland, Westphalen*, notices the formation of crystals of red copper-ore on a fragment of a Roman copper vessel which was dug up near to the city of Bonn. The inner and outer surface of the vessel was covered next to the copper with a delicate layer of red copper in small but beautiful dodecahedral and cubo-octahedral crystals; and immediately over this was an extremely thin layer of film of a green colour, and which might be considered as malachite. Nöggerath also observed, in a collection of antiquities at Triers, some wrought pieces of copper, several inches long and pretty thick, which were found amongst Roman ruins, and appeared to have served as architectural ornaments. They were so corroded on the surface, that little of their original form could be observed. In some places traces of gilding were visible. Under the green crust or ærugo was a layer of well-marked crystals of red copper. The Roman vessel found near Bonn appears to have been exposed to considerable heat; therefore the red copper, in that case, may have been the result of fusion: but no traces of fire could be detected in the copper relics of Triers, nor in that of some other specimens we shall now mention. Sage observed crystals of red copper on an old copper statue found in the Soane in the year 1766. Demeste mentions crystals of red copper he saw in the hollows of the fragment of the leg of a bronze horse, which had lain for some hundred years underground. Morveau says, the crystals were of two kinds; one

ruby-red, being red copper; the others emerald-green, and these were malachite: and Demeste adds, that some of the hollows also contained crystals of blue malachite or blue copper. Vauquelin informs us, that on examining the fragment of a statue which had been long buried, he found the exterior of red copper, and the interior of copper in its metallic state. It is evident that these changes in the copper, in the specimens just enumerated, had been produced by the action of the atmosphere and of percolating water. It is equally well known that similar changes have been produced on copper when fused under particular circumstances. Examples of this kind were met with in masses of copper inclosed in the lava which, in the year 1794, flowed over a considerable space of the district of Torre del Greco. Common copper coins were converted into red copper, and in some specimens the surface was crystallized. In some of the specimens of brass candlesticks from Torre del Greco, preserved in the museum of the University of Edinburgh, the zinc was separated from the copper. On some of them there are small brownish crystals of translucent blende, numerous octahedrons of red copper, and very beautiful copper-red cubes of pure copper. In other specimens from Vesuvius mentioned by authors, the zinc and copper have separated, and each appears crystallized in octahedrons; and also in the state of iron-glance and sparry iron, have been found in the lava of Vesuvius. Silver in beautiful octahedrons, lead in the state of litharge, and galena, or lead-glance, in the cubo-octahedral form, have also been collected from the lava of Torre del Greco.—Vid. Schweigger's Journal. —*Edin. Phil. Journ.*

ON VARIOUS PLANTS USED AS TEA IN DIFFERENT COUNTRIES.

The plants used as tea are as widely separated from each other as the countries themselves are remote. In Mexico and Guatimala the leaves of the *Psoralea glandulosa* are generally used as tea; and in New Grenada the *Alstonia theæformis* of Mutis (the *Symplocos Alstonia* of Humboldt and Bonpland) affords a tea not inferior to that of China. Further to the north on the same continent, a very wholesome tea is made from the leaves of the *Gaultheria procumbens* and *Ledum latifolium*. This last is vulgarly called Labrador tea, and its use was, I believe, first made known by the late Sir Joseph Banks. The most famous of all American teas, however, is the tea of Paraguay, of which large quantities are annually imported into Peru, Chili, and the States of Buenos Ayres; and the use of it is so universal in South America, that the inhabitants have always some of this tea ready prepared, whether engaged in occupations

occupations at home or in the fields, and no person departs on a journey without being provided with a quantity of the herb. It is made by merely pouring warm water on the leaves; and is sipped, through a silver or glass tube, from a small vessel called a Matè Pot, which is carried in the hand; or should the person be on horseback, or engaged in any occupation requiring the use of his hands, it is suspended from the neck by means of a small chain. It is frequently mixed with a little lemon-juice, and is used with or without sugar. European travellers with whom I have conversed prefer this to any of the teas imported from China. The Paraguay tea is the more remarkable from its being the produce of a species of holly, a genus hitherto considered as deleterious. It is described and figured under the name of *Ilex Paraguensis* in an Appendix to the second volume of Mr. Lambert's work on the genus *Pinus*, and is noticed by M. Auguste St. Hilaire in the "Mémoires du Museum," under the name of *Ilex Mate;* and by Drs. Spix and Martins, in their Brazilian Travels, under that of *Ilex Gongonha*. It has an extensive geographical range, being found in the extensive woody regions of Paraguay watered by the Parane, the Ypané, and Jejni, in the province of the Minas Geraes, and other districts of Brazil; and it appears to have been found in Guiana by M. Martin, as there are numerous specimens in his herbarium, part of which is in the possession of Mr. Lambert. We must believe these specimens to have been collected in the mountainous district, otherwise it would be impossible to reconcile the idea of the same plant being found in so different a latitude. The tree is about the size of the orange-tree, to which it bears considerable resemblance in its habits and leaves. The flowers are white, disposed in small cymes in the axils of the leaves. They are tetrandrous, and are succeeded by scarlet berries, like those of the common holly. The leaves, whether fresh or dried, are destitute of smell; but on a little warm water being poured upon them they exhale an agreeable odour. Mr. Lambert has been so fortunate as to obtain a living plant of this highly interesting tree, which is now growing in his collection at Boyton House, Wilts.—In New Holland the leaves of the *Corræa alba* make very good tea.—The inhabitants of those barren and remote islands denominated the Kurile Isles, in the Sea of Kamtschatka, prepare a tea from an undescribed species of *Pedicularis*, named by Professor Pallas in his herbarium (now in Mr. Lambert's possession) *Pedicularis lanata*.—It is unnecessary to take notice of all the aromatic herbs of the order *Labiata* used as tea in different countries: my object has been to show that teas are afforded

by

by plants very remotely separated from each other in point of affinity. But while on the subject of teas it may be interesting to observe, that the common black Chinese teas consist chiefly of the old leaves of the *Thea viridis*, mixed with those of the *Camellia Sasanqua* or *oleifera*, and sometimes fragments of the leaves of the *Olea fragrans;* and that the finest teas, whether green or black, appear to be produced by the *Thea Bohea*, the quality and colour depending solely on the age of the leaves and the mode of preparing them. Although I have long attended to the subject, I have never been able to detect, in those teas said to be adulterated, either willow or sloe leaves, or any thing else of British growth. It is probable that the leaves of the species of *Camellia* before mentioned may have been taken for sloe leaves. D.D.—*Edin. Phil. Journ.*

CAPT. SABINE ON THE ELLIPTICITY OF THE EARTH.

The results of different combinations of the experiments on the length of the pendulum which we gave in our last Number, with others, are expressed by Capt. Sabine in this table:

	Ellipticity.
From Captain Sabine's 13 stations	1 : 288·4
From these 13 and 8 stations of the French .	1 : 288·7
From these 13 and 7 British stations	1 : 289·5
From the mean of 5 stations near the equator and 6 in Britain	1 : 288·3
From the mean of 5 equatorial and the most northerly 5	1 : 288·4
From the 6 British and the 5 northerly . . .	1 : 288·5
From the general combination of 25 stations .	1 : 289·1
Mean	1 : 288·7

"The attempt," says Captain Sabine, "to determine the figure of the earth by the variations of gravity at its surface, has thus been carried into full execution on an arc of the meridian of the greatest accessible extent; and the results which it has produced are seen to be consistent with each other, in combinations too varied to admit a probability of the correspondence being accidental. The ellipticity to which they conform differs more considerably than could have been expected, from $\frac{1}{300.75}$, which had been previously received on the authority of the most eminent geometrician of the age, as the concurrent indication of the measurements of terrestrial degrees, of pendulum experiments, and of the lunar irregularities dependent on the oblateness of the earth."—*Quarterly Journal of Science.*

LIST

LIST OF NEW PATENTS.

To Thomas Steele, of Magdalen College, Cambridge, esquire, for improvements in the construction of diving-bells, or apparatus for diving under water.—Dated 28th Oct. 1825.—6 months to enrol specification.

To Vernon Royle, of Manchester, for improvements in the machinery for cleaning and spinning of silk.—1st Nov.—2 months.

To John Isaac Hawkins, of Pancras, for improvement in implements used in the manufacturing and preserving of books.—1st Nov.—6 months.

To John Ridgway and William Ridgway, of Cauldon Place, in the Staffordshire Potteries, manufacturers of earthenware, for their improved cock-tap, or valve for drawing off liquors.—1st Nov.—2 months.

To John and Samuel Seaward, of the Canal Iron-Works, Poplar, Middlesex, engineers, for improved methods of propelling boats and all kinds of vessels on canals, rivers, and other shallow waters.—2d Nov.—6 months.

To William Ranyard, of Kingston, Surrey, tallow-chandler, for his circumvolution brush and hander.—2d Nov.—2 months.

To Thomas Seaton, of Bermondsey, Surrey, shipwright, for his improvements on wheeled carriages.—5th Nov.—6 months.

To George Hunter, of Edinburgh, for an improvement in the construction, use, and application of wheels.—6 months.

To Thomas Shaw Brandreth, of Liverpool, barrister-at-law, for an improved mode of constructing wheel-carriages.—8th Nov.—6 months.

To Samuel Brown, of Old Brompton, Middlesex, for improvements in machinery for manufacturing casks or other vessels.—8th Nov.—6 months.

To William Erskine Cockrane, of Regent-street, Middlesex, for an improvement in cooking apparatus.—8th Nov.—6 months.

To John William Hiort, chief engineer in the Office of Works, architect and surveyor, for an improved chimney or flue.—8th Nov.—2 months.

To Charles Louis Giroud, of Lyons, in France, (at present residing at No. 13, Queen-street, Soho,) for a chemical substitute for gall-nuts in all the branches of the arts in which they are used.—8th Nov.—2 months.

To James Wilks, and John Erroyd, of Rochdale, Lancashire, for an improved engine for cutting nails, sprigs, and sparables.—8th Nov.—6 mo.

To John James Alex. M'Carthy, of Pall Mall Place, for an improved pavement or covering for streets, roads, &c.—10th Nov.—6 months.

To Benjamin Cook, of Birmingham, for a method of rendering ships' cables and anchors more secure and less liable to strain and injury while the vessel lies at anchor.—10th Nov.—6 months.

To Benjamin Cook, of Birmingham, for improvements in the binding of books and portfolios.—10th Nov.—6 months.

To J. George Deyerlein, of Mercer-street, St. Martin's in the Fields, smith and tool-maker, for improvements, communicated from abroad, on weighing machines, denominated German weigh-bridges.—10th Nov.—6 months.

To Samuel Parker, of Argyle-street, Middlesex, bronze and iron founder, and William Francis Hamilton, of Nelson-street, Long-lane, Surrey, engineer, for alloy or alloys of metals.—12th Nov. 6 months.

To Edward Bowring, of Goldsmith-street, London, silk-manufacturer, and Robert Stamp, of Buxted, Sussex, weaver, for improvements in the working, weaving, or preparing silk and other fibrous materials used in making hats, bonnets, shawls, and other materials.—17th Nov.—6 months.

To James Guestier, of Fenchurch-buildings, for a mode of making paper from substances thereby applicable to that purpose.—17th Nov.—6 months.

To Alexander Lamb, of Princes-street, Bank, London, and William Suttill, of Old Brompton, Middlesex, flax-spinner, for improvements in machinery for roving and spinning flax, &c.—17th Nov.—6 months.

To George Borradaile, of Barge-yard, Bucklersbury, for an improved method of making or setting up of hats or hat bodies.—17th Nov.—6 months.

A METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

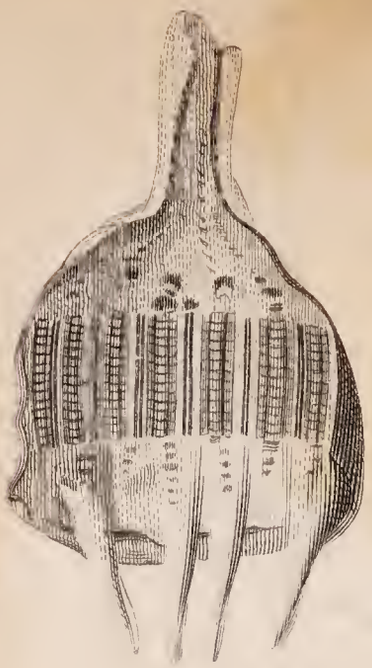
[The time of observation, is at Half-past Eight A.M.]

1825.	Thermometer.	Barometer.	Wind.	Weather.	Fall of Rain.
Oct. 26	37·	29·80	NW.	Fine	
27	35·5	29·77	NW.	Cloudy	
28	53·	29·66	NW.	Ditto	
29	53·	29·65	SW.	Ditto, rain P.M.	
30	50·	29·50	SW.	Ditto	0·02
31	49·5	29·60	W.	Fine	
Nov. 1	49·	29·55	SW.	Cloudy, rain A. M. & P. M.	
2	47·	29·39	SW.	Ditto, rain A. M. & P. M.	·06
☾ 3	52·	28·52	SW.	Ditto	·10
4	38·	29·23	SW.	Ditto	
5	36·5	29·70	W.	Fine, rain P.M.	
6	47·	28·80	S.	Ditto	·15
7	35·5	29·	SW.	Ditto, snow P.M.	
8	34·	29·12	SW.	Cloudy, rain P.M.	
9	43·	28·72	W.	Fine	·53
● 10	43·	28·75	NE.	Rain	·09
11	39·	29·23	W.	Cloudy, rain P.M.	·06
12	32·	29·60	W.	Fine	·49
13	31·	29·75	NW.	Ditto	
14	41·	29·60	SW.	Ditto	
15	36·5	29·92	W.	Ditto	
16	34·	30·	W.	Ditto	
☽ 17	30·	29·90	W.	Foggy, rain P.M.	
18	37·	29·70	W.	Fine, rain P.M.	·15
19	40·	29·60	SW.	Ditto	·05
20	36·5	29·90	W.	Ditto	
21	54·	29·40	SW.	Rain	·15
22	41·	29·55	W.	Fine	
23	34·5	29·80	W.	Ditto	
24	44·	29·87	W.	Cloudy, rain P.M.	
○ 25	37·5	29·92	W.	Fine	·29
Average	41·	29·50			2·14

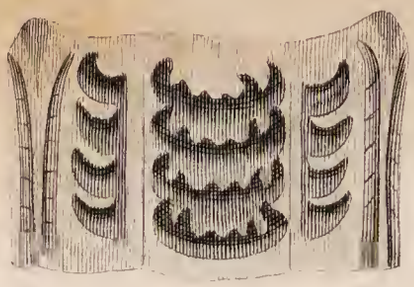
We have to regret that the indisposition of our valuable correspondent Dr. Burney has deprived us of the Gosport Register for this month. And the decease of our much-respected friend Mr. Cary, who for a very long time kindly supplied our Journal with his meteorological observations, has occasioned a similar deficiency with regard to London.

In our next volume we propose to give the register of each month entire; considering this to be more useful than the bringing down the observations so near to the day of publication.—Ed.

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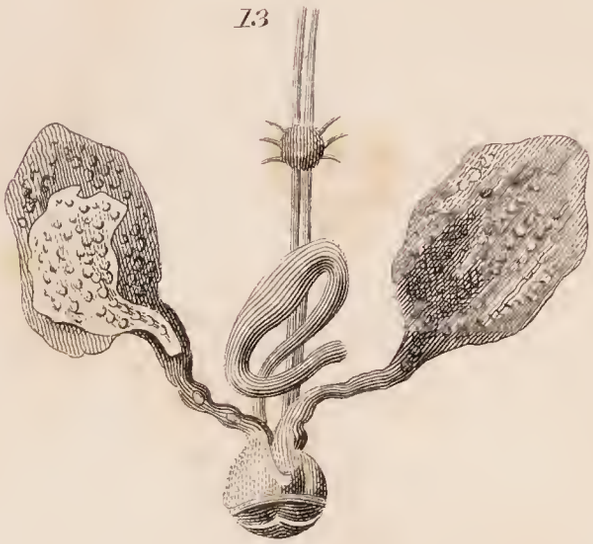
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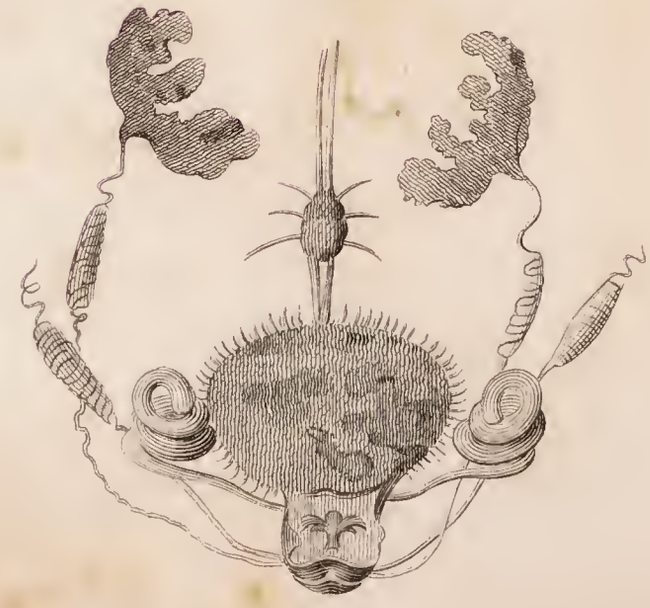
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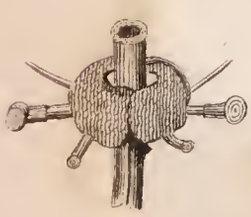
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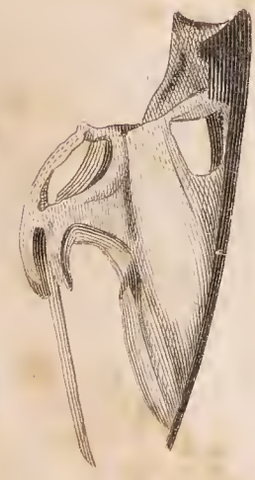
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THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *DECEMBER* 1825.

LXIII. *On the Anatomy of the Mole-cricket. By* J. KIDD, *M.D. and F.R.S. Reg. Prof. of Medicine in the University of Oxford*.*

THE following observations contain the principal points of a laborious examination of the anatomical structure of the gryllotalpa or mole-cricket; and if I dare hope that that examination has been conducted with any thing like adequate accuracy, I need not apologize for the length of the details with which the account of it is accompanied, since Cuvier has affirmed of an entire volume written by Lyonnet on the anatomy of a single species of caterpillar, that it contains not one word that is useless.

Natural science indeed has now arrived at that point, in which individual detail is requisite for the acquisition not only of a surer basis of classification of species, but also of more correct principles of general physiology. Independently however of these considerations, the insect which is the subject of the present communication is so singular in its structure and habits, and is in some parts of the world so formidable to the agriculturist, as to render its history peculiarly interesting.

It is described under various names; as the Earth-crab, from its general appearance; *Vermis cucurbitarius*, from the mischief it does to cucumber-beds. By the French naturalists it is called *Courtilière*.

The best account of the mole-cricket which I have met with is in a well known entomological work by Rösel, published at Nuremberg in 1749. This account is accompanied by the best engravings also of the external characters of the animal in its different states: and the value of these engravings is greatly enhanced by the accuracy with which they are coloured.

Rösel says that about the month of June or July, rarely later, the gravid female gryllotalpa excavates a cavity, from

* From the Philosophical Transactions for 1825, part ii.

four to five inches beneath the surface of the earth, in which she deposits her eggs in one heap, to the number of three hundred or more; and dies within a few weeks afterwards. At the end of about a month the young mole-crickets are produced, and appear, on a hasty survey, to bear a general resemblance to the ant. Between the time of their birth and the commencement of winter the young animals cast their skin three times: they lie dormant during the winter, deeper in the earth in proportion to the inclemency of the season; and during this period cast their skin for the fourth time. About May they leave their winter-quarters, and at this time are furnished with the rudiments of their future wings, four in number; which differ remarkably in size and form and position from those of the perfect insect; in which the inferior wings are folded in a very curious manner, while in the imperfect insect they are always open.

During the month of June or July they cast their skin for the fifth and last time; after which the wings acquire a permanent character, and the insect becomes capable of propagating its species.

Rösel says that he himself never dissected a mole-cricket; but reports, on the authority of others, that its stomach resembles that of the locust, represented in his ninth plate of the series of that tribe of insects. I may here add, from my own observation, that it very closely resembles that of the *Gryllus viridissimus*, and also that of a species of gryllus preserved in the Ashmolean Museum, which answers to the *Pneumora* of Lamarck: it also somewhat resembles that of a locust marked 614 in the Hunterian collection; and still more that of the Cape grasshopper engraved in the 84th plate of the first part of Sir E. Home's Comparative Anatomy.

It appears from Rösel's account, that while very young these insects are gregarious, but not afterwards: that they are usually found in the vicinity of meadows and of fields of corn, particularly of barley; to which they are very detrimental by feeding on the roots, and thus intercepting the due nourishment of the plants themselves. I have no doubt of the general accuracy of the foregoing remarks of Rösel, and have little to add to his account of the natural history of this insect. I have hitherto met with the mole-cricket in one situation only; namely, in some peat-bogs, at the distance of a few miles to the west of Oxford. In the neighbourhood of these peat-bogs the insects are familiarly known by the name of Croakers, from the peculiar sound which they occasionally make; a sound not very unlike, but more shrill and more soft than that of the frog. This sound, even in the case of a single individual, may

may be heard at the distance of some yards; but when made by numerous individuals at the same time it may be heard, as I have reason to believe, at the distance of some hundred yards, provided the air be in a favourable state. I have usually found the insect within a foot and a half of the surface, and in parts where the peat was neither quite dry nor very moist; of such a consistence indeed as is most favourable to the mining operations of the animal.

The accounts of different authors differ as to the food of the mole-cricket. Having kept several individuals in glass vessels during some weeks, I observed that of all kinds of vegetable food they preferred the potatoe, while cucumber they hardly touched; but if raw meat were offered them they attacked it with great greediness, and in preference to every thing else. And when they had been kept, though even but for a short time, without any food, they did not hesitate to attack each other; in which case the victor soon devoured the flesh and softer parts of the vanquished. As I have not unfrequently found them in their native haunts maimed in various parts of the body, I have very little doubt that, although captivity may increase their ferocity, they are not, even in a natural state, free from each other's attacks. If they are carnivorous, they probably feed on worms and various larvæ, which are abundant in the peat-bogs above mentioned, for I have repeatedly found the horny and indigestible parts of insects within their stomachs. Similar relics I have found in the stomach of the *Pneumora* and *Gryllus viridissimus*. The two following facts attest in the tribe of insects to which the mole-cricket belongs a remarkable degree of voracity, and an equally remarkable power of abstaining from food. My friend Dr. Macartney, of Dublin, informs me that he has known a gryllus devour a portion of its own body. On the other hand, my friend Mr. Buckland, of this university, gave me, at the commencement of the present summer, a living gryllotalpa, which had been confined during nine or ten months in a tin case containing a small quantity of garden mould, without the possibility of having met with any other nourishment than such as that portion of mould might be supposed to contain.

External Characters of the perfect Gryllotalpa.

In this, as in the case of every other animal with whose habits of life we are acquainted, we see a perfect accommodation in form and structure to the circumstances in which the individual is naturally placed. Destined like the common mole to live beneath the surface of the earth, and to excavate a passage for itself through the soil which it inhabits, the gryl-

lotalpa is furnished like the mole with limbs particularly calculated for burrowing; with a skin which effectually prevents the adhesion of the moist earth through which it moves; and with exactly that form and structure of body by which it is enabled to penetrate the opposing medium with the greatest ease. At the same time, in order to prevent the necessity of its excavating a track so wide as to admit of the body being turned round in case of a desire to retreat, it is endued with the power of moving as easily in a retrograde as in a progressive direction; and, apparently to perform the office of antennæ, which warn the insect of approaching danger in its progressive motions, it has two appendages, which might not improperly be called caudal antennæ, evidently calculated to serve a similar purpose during its retrograde motions; particularly as they are furnished with very large nerves. The indifference with which the insect is disposed to move in either direction is manifested by the following experiment:—If you touch it towards the head, it retreats; if towards the other extremity of the body, it advances.

The general colour of the animal is such as indirectly to serve as a protection to it, being nearly of the same hue as the vegetable mould in which it lives; so that it is not very readily distinguished upon being first turned up to view; and its safety seems to be still further insured by the appearance of death, which, in common with many other insects, it assumes when suddenly disturbed. This stratagem, for so it may be called, appears to be most decidedly practised by the animal while in captivity; and if thrown at random out of the vessel in which it has been confined, however unnatural the posture may be into which it has been thrown, it remains as it were in a state of catalepsy during half a minute or more: the first indication which it gives of recovery from this stupor, invariably consists in a motion of the extremity of the antenna.

The general colour of the insect is a dusky brown, passing either into a reddish brown or into an ochry yellow; those parts being of the darkest colour which are most exposed to view when the animal is moving in the open air. Every part of the body is to a greater or less degree covered by a kind of down, which seems to be the efficient cause of its capability of repelling moisture; which capability is so remarkable, that when the insect is plunged under water it appears as if cased in silver, or some bright metallic covering; this appearance being evidently derived from a stratum of air interposed between its body and the surrounding liquid. This down not only serves to repel the adhesion of any moist substance to its body,

body, but also facilitates the motion of the animal, by lessening the degree of friction which would otherwise take place; and it is owing to the same circumstance that there is an unusual degree of difficulty in retaining a sure hold of the insect, even when dead; but more especially when alive, and struggling against detention. The degree of force which it commonly exerts on such occasions is very remarkable; and, from the sensation produced, may easily be supposed to be what Rösel says it is, equal to the counterpoise of two or three pounds. The skin or covering of the insect is in some parts nothing more than a thin membrane; in other parts it resembles soft leather; and sometimes equals horn or even shell in its degree of hardness.

The mole-cricket is more distinctly divisible than most other insects into three separate parts, which I will call respectively the head, the thorax, and the abdomen, although I am aware that the anterior part of that which I call the abdomen is usually considered as a part of the thorax. Of the three parts above mentioned, the head is not above one twelfth the length of the whole body; the thorax three twelfths; and the abdomen eight twelfths.

The head is united to the thorax, as the thorax also is united to the abdomen, by means of a loose membrane, which envelopes the muscles that pass respectively from one to the other; and it is in consequence of the looseness of these membranes that the animal is enabled either to separate the connected parts to a considerable distance from each other, or to contract them so closely together as to hide the interposed membranes from view; and, from the arched form of the anterior part of the thorax it can draw in its head under that part, much after the manner of a tortoise. The same flexibility of the connecting membranes enables the animal to place either its head or its thorax at a considerable angle with the rest of the body; a movement which is very characteristic of this insect, and gives it an air of intelligence, the attitude being apparently that of watching, or listening.

*The head**.—All the upper part and the sides of the head form a hard, thick, horny case, containing the various muscles which move the jaws; and, in order to strengthen this case, two firm bars run transversely across the bottom both of the anterior and posterior margin; which bars are themselves united together by a still stronger bar or beam, which runs longitudinally from the middle of the one to the middle of the other. There is nothing very remarkable in the parts which

* Vide Plate II. figs. 1 and 2.

constitute

constitute the mouth, excepting the maxillary and labial palpi. In the maxillary palpi there are five joints or parts; in the labial there are three; and the last of these joints in each of the palpi terminates in a rounded extremity, like a pestle: this extremity, which is of a honey-yellow colour, is perfectly smooth, while every other part of the palpi has a rough and hairy surface. In their natural position the palpi are bent and projected forward, so as to resemble the fore legs of a horse in the act of cantering.

The antennæ, which are situated near the articulation of the mandibles, consist of a great number of minute segments, resembling beads of a circular form. The number of these beads, which varies in different instances, is usually from 100 to 110; rarely more or less: but it is worth noticing that in examining the two antennæ of the same individual, I sometimes found the number of beads greater in one than in the other; and as the terminal bead differs in its form from all the rest, the result of the examination is less open to doubt than it would otherwise have been. Each bead is united to the one that precedes and the one that follows it, by means of a soft, white, very flexible membrane; in consequence of which, and of the number of the joints, the insect can move and bend the antennæ with great facility in every direction, excepting at the very root: there the motion is confined by a ridge that only admits of its being directed from behind, forwards, or *vice versâ*.

The anterior edge of each bead is fringed with bristly hair; which, surrounding the joint that connects it to the following bead, gives to the whole, when viewed by a magnifying lens, the appearance of a sprig of *Equisetum*. The beads are upon the whole larger, in proportion as they are nearer to the origin of the antennæ; but here and there, and without any regularity in the variation, one of the beads is either much larger or much smaller than those in the vicinity.

Whatever be the primary use of the antennæ and palpi (on which subject entomologists are not agreed), their general importance is allowed by all, and is evinced in the particular instance now before us by the extraordinary attention bestowed upon them by this insect. Those who may be led to watch its habits will repeatedly observe the antennæ bent forwards and downwards, by a curious application of the fore legs towards the mouth: and then by a regulated motion, not unlike that by which the resin is applied to the bow of a violin, they are passed between the maxillæ; in order, as it would appear, either to moisten the organs, or to disengage from their surface particles of dust or other extraneous substances which may have accidentally adhered to it. With a more rapid motion

tion the insect from time to time dresses, if I may use the expression, its palpi; bending them inwards and brushing the surface of their extreme parts by a frequent application of the maxillæ. A similar care of the antennæ and palpi is observable in the *Gryllus viridissimus;* with the additional circumstance, that that insect very often passes between its maxillæ the curiously padded surfaces of its feet, much in the same manner as a cat licks its paws.

*The eyes**.—The gryllotalpa has two compound eyes, as they are called, and two ocelli or stemmata. Latreille uses this expression "*ocellus medius subobiteratus;*" from which it may be inferred that he supposes the ocelli to be three in number; but after the most careful examination I have not been able to discover more than two. The compound eyes are situated immediately behind, but a little exteriorly to the antennæ: the corneæ of these eyes, which are large in proportion to the size of the head, are segments of a sphere, flattened however on the inner side so as to present a vertical plane surface to a similar plane surface in the opposite eye; and it is remarkable that this part of the cornea, and the mere margin of the rest of it, are the only parts capable of freely transmitting light: all the remaining portion is covered, on the interior surface, by an opaque pulpy membrane, or pigments of a mulberry-colour; yet the portion obstructed by this pigment is in itself nearly as transparent as flint-glass; it is studded over on the interior surface with numerous depressions of a circular form, which, being very closely set together, give it a reticulated appearance.

The stemmata are placed between the middle of the compound eyes, so as to be rather further from each other than from the eye of the same side. They are not so large as a very minute pin's head, of a lenticular form, perfectly transparent, but not quite colourless, resembling particles of very pale cairngorum quartz. In two instances I have found only one of the stemmata, without any trace of the other. An anomaly somewhat of the same kind has been observed by the father of my friend Dr. Ogle, of this university, in the case of a man; on one side of whose breast the usual rudiments of a mamma were entirely wanting.

With respect to the small quantity of light admissible through the corneæ of the eyes of the mole-cricket, it is apparently sufficient for the purposes of an animal living almost constantly underground. The spherical form of that part of the corneæ which is itself incapable of transmitting light is probably intended, (as was suggested to me by Mr. Whessel,

* Vide Plate II. figs. 1 and 2.

to

to whom I am indebted for the principal drawing which accompanies this paper,) as a protection for the vertical transparent portion.

*The thorax**.—The form of this part is that of an irregular cylinder, passing into a cone towards the anterior part: the upper portion and the sides, which are covered with a remarkably smooth down resembling the finest velvet, form a horny case of considerable thickness and strength; which contains, or, more properly speaking, is almost entirely occupied by the very large and powerful muscles which move the fore legs. It is divided longitudinally into two equal parts, by an almost bony septum of a complicated form: this septum upon the whole bears an obvious resemblance, but in an inverted position, to the deep sternum, together with the furcular clavicle of birds; and is destined indeed to a similar use,—to give attachment to the powerful muscles which are to move the anterior extremities. It differs however from the corresponding part in birds in two considerable points. It differs, first, in consisting of two laminæ instead of one: these laminæ are parallel to, but distinctly separated from each other, so as to give passage to the œsophagus, and room for the attachment of muscles which assist in moving the adjacent parts. It differs again from the sternum of birds by having a very hard spine, which resembles a common thorn, attached to the inferior and posterior edge of the furcular bone, and passing rather obliquely downwards and backwards. This process serves for the attachment of numerous muscles which adhere very firmly to it, and are inserted on either side of the commencement of the abdomen; enabling the animal to bend its thorax to an angle with the abdomen, a posture which has already been described as very characteristic of this insect.

From the under part of the thorax and near its posterior extremity arise the two fore legs, those singular instruments which so peculiarly characterize the mole-cricket. Compared indeed with the other legs, and with the general size of the animal, they are as if the brawny hand and arm of a robust dwarf were set on the body of a delicate infant; and the indications of strength which their structure manifests, fully answer to their extraordinary size:—but I shall describe them more particularly hereafter, and proceed now to the description of the abdomen.

The abdomen†.—In its general form and structure this part resembles the corresponding part of the hornet; but it consists of more segments, and is much less bright in colour.

* Vide Plate II. figs. 3 and 4. † Ibid. fig. 2.

There

There are twelve segments in the abdomen of the gryllotalpa, of which the nearest to the thorax carries the upper pair of wings on its upper part, and the middle pair of legs on its lower part; the next segment carries the under pair of wings on its upper part, and the hind pair of legs on its under part. These two segments, which are usually described in entomological systems as belonging to the thorax, are of a horny consistence and very hard on their upper side, while all the rest are merely membranous; they are also covered with much long and rough hair, while all the rest, excepting the last but one, are sparingly covered with short hairs. The last segment but one is furnished on each side of its upper surface with a row of red hairs or bristles, which are curved inwards in a direction towards each other; obviously for the purpose of preventing the folded extremities of the under wings from falling off the back on either side.

The under surfaces of all the segments are of a thicker substance than the upper, and are covered entirely with a coarse down, which probably gives the animal a more firm hold while in the act of burrowing. In the last segment is situated the vent, formed by three oval flaps, two below and one above. This segment sends out from each side of its upper surface two caudal antennæ, as I have ventured to call them, of a tapering form, which differ essentially in structure from those of the head, inasmuch as they are not jointed in any part of their extent, excepting at their very commencement: they are furnished with short hairs set comparatively closely about every part; among which are interspersed long single hairs. These caudal antennæ are evidently very sensible, and serve probably to give the animal notice of the approach of any annoyance from behind; they are partially hollow throughout great part of their extent, and muscles may be traced into them from the inner and adjoining part of the abdomen.

The legs.—The anterior legs, passing out from under the hind part of the thorax, advance by the side of the head in a direction parallel to each other, which is their natural position while the animal is at rest. I should deem it a servile adherence to system were I to describe the parts composing these legs by the terms strictly indicative of the order of their succession; for, thus, that part which answers so eminently to the character of a hand, must be called the tibia. I shall beg leave therefore to state principally, that the fore leg of this insect consists of three main parts, with a lateral appendage attached to the last of them. The two first of the three parts bear some general resemblance to the claw of the crab; being short and thick, for the purpose of affording room for power-

ful muscles, intended to move the last part; which is the immediate instrument employed by the animal in burrowing.

It might I think be asserted, without the fear of contradiction, that throughout the whole range of animated nature there is not a stronger instance of what may be called intentional structure, than is afforded by that part of the mole-cricket which I am now to describe*.

The natural and constant position of this member is worth noticing; the palm, as it may be called, facing outwards, and the claws ranging not in a horizontal but a vertical line, so that none of them but the lowermost, and not even this necessarily, touches the surface on which the animal is walking. Accordingly the insect does not make much use of its fore legs in walking; and, if irritated, it advances towards you with these legs elevated, in a menacing attitude as it were; not unlike the corresponding attitude of the insect called the *Mantis*. The form of the hand is that of a triangle; the base of which is formed by the four claws, while the apex is situated at the joint connecting this with the preceding part: by which form and disposition, two important objects are gained; for the joint is thus capable of a much greater extent of motion than it could have possessed had the articulating surface been more than a mere point; and at the same time, the greater extent of the base enables it to act with more powerful and more rapid effect than could have been otherwise produced. The four claws which form this base constitute the proper burrowing instrument; and their shape and structure are beautifully adapted to the purpose: for instead of being covered with down or hair, like all the rest of the limb, they are hard, and have a perfectly polished surface; doubtless in order to prevent as much as possible the adhesion of the earth through which the animal is to make its way: they have each of them sharp but strong points, which proceeding from a broad base are thus rendered more effectual. In each also of the claws one of the edges is sharp, while the other is comparatively blunt; and all the cutting edges, as also the terminating points, are directed downwards. Their outer surfaces are slightly concave both in the longitudinal and transverse direction; so that all together they form a scoop as it were, by which the earth that has been scraped off by the points is moved out of the way. They are also each of them divided longitudinally on their concave side by three or four slight ridges; so that, though highly polished, their surface is not absolutely smooth: and thus being concave and uneven, they are more apt to re-

* Vide Plate II. fig. 5.

tain

tain particles of the excavated earth; which, by filling up the indentations of the claws, would necessarily impede their due action. To obviate this inconvenience, an exceedingly curious instrument is attached to the upper part of the concave surface of this member: this instrument consists of two claws, closely resembling those already described, having by their side a small brush as it were, which terminates in two spines. These two claws, together with the piece bearing the spines, arise from a single piece, or handle, which is articulated in such a manner as to move in a plane parallel to that in which the four claws are placed, but in a direction opposite to that in which they are moved: they are also placed in such a manner that their points and cutting edges are opposed to the points and cutting edges of the true claws; and hence the two parts, thus opposed to each other, act like the blades of a pair of shears. When first I considered this mechanism, and remembered that in the localities where I had found the animal the earth was frequently traversed by fibrous vegetable roots, which must necessarily retard its progress, I supposed that it used this instrument as a pair of shears to cut through those fibres. It is Rösel's opinion, however, that the instrument is intended to clear the true claws of the dirt that may from time to time collect upon and clog them; and unless both opinions be true, Rösel's appears the more probable.—But I have not yet concluded the account of the curious mechanism of this member: for the brush which has just been described, has only such an extent of motion as enables it to clear the two uppermost claws, or at most the three uppermost; the two lowermost however may effectually be cleared by a kind of feathered spur, which, arising from the further extremity of the joint answering to the femur, proceeds directly towards the lowest part of the burrowing instrument, and is easily made to sweep over the surface of the two last claws by bending the intermediate joint, the only difference in its mode of action being that it passes over their inner instead of their outer surface.

The middle pair of legs, which is the smallest of the three pairs, arises from the under part of the first segment of the abdominal division: they pass out from the body at right angles to the abdomen, and usually are seen in that direction whether the animal be in motion or at rest. They consist each of four parts; a very short coxa, a femur and tibia nearly equal in length to each other, and a tarsus which consists of two long and an intermediate short joint, the last joint terminated by two curved spines. There are several sharp, hard, straight spines near the angle made by the union

of the tibia with the tarsus; some of which being directed downwards, give the insect a firmer hold in walking.

The hind legs bear a general resemblance to the middle legs; but the coxa, femur, and tibia,—the femur especially,—are much larger and stronger. The relative position of the parts with respect to each other is the same as that of the middle legs; but their general direction, instead of being at right angles to that of the abdomen, is parallel to it. In addition to several sharp spines placed about the joint of the tibia and tarsus, and directed downwards as in the middle legs, there are four or five others placed at the back of the tibia near its lower extremity, and pointing slightly downwards. The structure of the tarsus scarcely differs from that of the middle leg. These hind legs are evidently the great instruments of progressive or retrogressive motion.

The wings.—There are two pairs of wings: the upper pair, arising from each side of the first segment of the abdomen, partially cover the lower pair, which arise from each side of the second segment. In several instances I found adhering to the body, in the vicinity of the roots of the wings, a minute parasitic insect of a light scarlet colour: the number of these parasitic insects rarely exceeded eight or ten in the same mole-cricket, but in one instance I counted nearly forty*.

The upper wings in the full-grown mole-cricket are not above one-fourth the size of the other pair: they are of an oval form and convex externally; and their nervures or wing-bones, as they are called by Dr. Leach, are remarkably thick and hard.

The under wings, when expanded, measure full three inches from the outer extremity of one to the corresponding extremity of the other. They may be compared in form to a bivalve shell, contracted and elongated towards the hinge; at which point is the joint of the wing: from hence, as many as thirty nervures, almost all of which are remarkably delicate, radiate in straight lines to every part of the extremity. A very thin and nearly colourless and transparent membrane forms the medium through which these nervures radiate; and throughout the whole expanse of the wing these nervures are mutually united by more delicate nervures, which cross at nearly regular intervals, and at right angles from one to the other, presenting altogether the appearance of a curiously chequered surface. These wings, though so broad when expanded, are scarcely the twelfth of an inch in breadth when folded; and appear at first view, in this state, any thing but what they really are. They have indeed been often mistaken for a mere

* Vide Plate II. fig. 5 *a*.

caudiform

caudiform appendage to the other wings, from under which they emerge. When folded, (and they fold themselves longitudinally like a fan,) their very delicate texture is protected by the following simple contrivance. In each wing the two exterior longitudinal nervures, with their intervening membrane, are comparatively strong and thick; and these form the lateral walls of the wings when folded.

In each wing also there are two other nervures not far from the former, and circumstanced like them with respect to strength; which, when the wings are folded, close together so as to form a horizontal covering, or roof, of sufficient strength to protect the subjacent membrane from ordinary accidents. As the narrow case formed by the wings thus folded extends beyond the extremity of the abdomen, and might easily slip off so convex and smooth a surface, such an accident is guarded against by the contrivance already described, namely, an apparatus of hairs or bristles placed on either side of the upper surface of the last segment but one.

The digestive organs *.—It is mentioned in the 48th letter of White's Natural History of Selborne, on the authority of anatomists who have examined the intestines of the mole-cricket, that "from the number of its stomachs or maws, there seems to be good reason to suppose that it ruminates, or chews the cud like many quadrupeds." A cursory view of these parts, however, is enough to show that such an opinion could only have been deduced from some very general points of resemblance; and the probability of its truth is entirely destroyed upon an examination of their internal structure.

In fact, the digestive organs of this insect resemble more closely those of a granivorous bird than of any other animal, as will appear from the following description. The œsophagus, which on its upper side is blended with and forms a continuation of the inner surface of the upper lip, commences on the lower surface in a loose corrugated tongue as it were, which is attached at its base to the inner surface of the lower lip: from hence it is continued along the under part of the head and neck, and between the bony laminæ of the sternum, in the form of a distensible and longitudinally folded tube of a reddish-brown colour; it then passes on among the muscles of the two hind pair of legs, and at length terminates in a very large crop of an oval form. In the vicinity of the mouth it is surrounded by muscles which arise from its outer coat, and are inserted at nearly right angles into the adjacent parts; these muscles of course serving to open and distend it.

In the crop itself two sets of muscular fibres are very easily

* Vide Plate II. fig. 6.

discernible,

discernible, some running in the direction of its length, others surrounding it in the opposite direction; and it is lined by a very thin membrane having a cuticular character.

The tube which passes from the crop towards the intestines commences so near the termination of the œsophagus, that externally it appears to be a continuation of the latter; it is very thick and strong in comparison with its diameter, and consists of a coat of muscular fibres disposed circularly, lined by a membrane which has evidently a glandular character. This tube terminates at a short distance from its commencement in a small organ, scarcely larger than a hemp-seed, which may very properly be called a gizzard; though more complicated in its structure, and more effectual for the intended purpose than the gizzard of any bird.

The form of the gizzard is nearly spherical, and it consists of a thick external muscular coat, which is lined by a glandular membrane of very singular construction, the inner surface being divided longitudinally into six equal parts, separated from each other by two horny ridges of a dark-brown colour: each division is furnished with three series of serrated teeth, of the consistence of tortoise-shell, and nearly of the same colour, running from the top to the bottom; of which those of the middle series are twice as broad and more complicated in form than those of the lateral series. As there are fifteen teeth in each of the three series of the six divisions, the gizzard contains in the whole 270 teeth *. In separating the muscular coat of the gizzard from that which lines it, (which may be easily done by maceration,) the exterior surface of the glandular coat in which the teeth are inserted is exposed to view. The appearance of this surface is very singular, and may be compared to a piece of fine lace-work, of which the meshes represent the intervals of the inserted teeth, the parts of the membrane in which the roots of the teeth are inserted resembling the lace-work itself.

Four of the divisions above described are elongated so as to terminate in a tapering membranous appendage, consisting of a natural fold, which serves to convey onwards any fluid particles that may have been pressed out by the action of the gizzard; and these four appendages so collapse together as to form a point as it were, which lies immediately in contact with the commencement of the common intestines. This apparatus is only discoverable by dissection; for it is contained in a large membranous cavity of the shape of a horse-shoe, the base of which passes across the lower extremity of the giz-

* Vide Plate II. fig. 6. Plate III. figs. 7 and 8.

zard,

zard, while the sides form two enormous cæca, which ascend obliquely outwards on each side of the gizzard.

As the muscular compression of the gizzard must necessarily have a tendency to force a part of any expressed fluid back into the œsophagus, we may expect this organ to be so constructed as to prevent such an effect; and it is probably for this purpose that its upper part is furnished with several projecting papillæ, each terminating in a small horny particle; which, like the sesamoid particles in the semilunar valves of the human aorta, may serve to complete the valvular action of the papillæ to which they are attached.

The cæca which have been above described are traversed longitudinally by several very broad duplicatures of their internal membrane; and judging from their usual contents, these appendages of the intestine are destined to receive and to perfect the digestion of those particles of food from which the gizzard has pressed out the liquid contents; and while, by means of the membranous folds already described, the expressed fluid is conveyed immediately into the mouth of the intestinal canal that passes from the general cæcal cavity, the cæca themselves receive the solid compressed particles which are forced out laterally at the extremities of those two divisions of the gizzard, which, having no membranous fold attached to them, leaves thus a vacant interval for the passage of the undigested mass. That this opinion is correct may be presumed, not only from the very mechanism of the parts, but from the state of the contents of the cæca, which are of a less crude character than the contents of the crop, and of a more crude character than the contents of the portion of intestine immediately beyond them. A strong confirmation of the foregoing opinion is obtained from a comparison of this part of the anatomy of the mole-cricket with that of the corresponding part in the ostrich; the stomach of which bird, acting like a gizzard by means of numerous pebbles which it takes into that organ, is aided by two enormous cæca, which, though they are not immediately in contact with the stomach, are not far removed from it; and, like the stomach, contain numerous pebbles, which are both smaller and smoother than those of the stomach itself, as being only destined to act on food already partially digested. The analogy on which I have just insisted, is strengthened by the fact that there are very large duplicatures of the internal coat of the cæca of the ostrich, as in the corresponding parts of the mole-cricket. I either therefore misunderstand or cannot agree with M. Marcel de Serres, (the author of a very interesting paper on the Intestinal Canal of Insects, published in the 76th volume of the *Journal de Physique*);

Physique); who seems to attribute to the cæca above described the office of an hepatic organ, and calls them "*vaisseaux hépatiques superieures*," in contradistinction to another organ situated lower down in the intestines, and acknowledged by all to be of an hepatic character.

From the common base of the two cæca a very narrow but powerfully muscular tube, which might with much propriety be called the jejunum, passes onwards for a very short space, and terminates in a large intestine: this intestine, which is eight or ten times the diameter of the jejunum, contracts very gradually as it proceeds, till, near the extremity of the rectum, it swells out very considerably. This large intestine is slightly convoluted in its course, and is usually more or less distended with a black pasty matter resembling soft clay. Among the contents of the upper part of this large intestine were almost invariably found from ten to twenty worms, of a white colour, and of a shape resembling the *Lumbricus teres* of the human intestines, but thicker in proportion to their length, and narrowing more suddenly towards their caudal extremity. In all of these worms the common intestines were distinctly visible through the integuments; and in many of them were distinctly visible also from ten to fifteen ova *.

On opening and removing the contents of the upper portion of the great intestine, four rows of minute bodies of a glandular character †, and of nearly a black colour, are brought into view ‡; two of which rows originate from the very commencement of the great intestine, and pass downwards through more than half its course: exteriorly to these two rows are two others, one on each side, which are parallel to the preceding, but originate at some distance from the commencement of the intestine. Immediately below the termination of this glandular apparatus is a small opening, very readily distinguishable on the inner surface of the intestine; which is the orifice of a cylindrical tube of a white colour, and of about the size of a horse-hair. This tube, after having been traced a short distance in a direction towards the gizzard, is lost in a mass or brush of still smaller tubes of an exceedingly bright yellow colour; these tubes, which amount probably to 150 or 200 §,

* Vide Plate II. fig. 9.

† The only doubt which I entertain as to the glandular character of these bodies arises from a reliance on the authority of Cuvier, who says "that the glands of insects are in every instance nothing more than parcels of free tubes floating in the interior of the body, and held together by the tracheæ." *Journ. de Phys.* tom. xlix. p. 344.

‡ Vide Plate III. fig. 9 *a*.

§ Cuvier states in the *Journal de Physique*, tom. xlix. p. 346, that the number of these tubes in the gryllotalpa amounts to many hundreds; but I feel certain that he greatly overrates the number.

are

are partially coiled round the contiguous viscera so as not to be very easily disentangled. A similar organ is represented in Sir Everard Home's Comparative Anatomy, vol. i. pl. 84, as belonging to the Cape grasshopper: it was originally considered by Mr. Hunter, and is considered generally at present, as answering to the liver of the higher classes of animals.

Each of these tubes springs out of a common cavity in which the white tube from the intestine terminates; but at their free extremity they are all impervious. Each tube appears partially filled with a granular pulpy substance which is almost universally of a bright yellow colour, though sometimes a particle is visible here and there of a clear light green colour; and I have seen similar green particles in the duct leading from the intestines.

The following peculiarity is observable in the individual structure of these tubes: their diameter for about one third of their course from the closed extremity is very small, and they are colourless and apparently empty; after which they suddenly undergo a considerable enlargement, become yellow, and are partially filled with the contents above described.

Maceration in water destroys the yellow colour in the course of a few minutes; from whence it may be inferred that after death the colouring matter transudes through the tubes containing it,—a circumstance observable also with respect to the biliary vessels of the higher orders of animals; but it seems certain that no such transudation takes place during the life of the animal; for, upon examination of the insect soon after death, I have never found the adjacent parts coloured, as they would have been by the escape of the contents of the tubes.

The portion of the intestine below the orifice of the hepatic duct, as it may be called, appears to be externally traversed in a longitudinal direction by several rows of small convex eminences resembling beads; these are the outer surfaces of so many corresponding internal sinuses, which are probably formed as the similar sinuses in the large intestines of man, and many other animals, by a peculiarity in the disposition of the fibres of the muscular coat.

Near the termination of the intestine are two orifices, one on each side, communicating each with a duct which soon swells out into a vesicular bag: these bags may probably be glands that secrete the fetid matter which the insect ejects from the anus when irritated. In one instance I found, on the site of the orifices above mentioned, two small bodies about the size of a pin's head, of a dark colour, and to the naked eye of a spherical form: my surprise was considerable when,

upon observing them with a magnifying lens, I perceived that they exactly resembled a crystallized rosette of brown pearl-spar. Upon being removed and submitted to the requisite experiments, they proved to be of considerable hardness, sparry in their structure, and insoluble either in boiling water or alcohol; but they were dissolved with rapid effervescence in diluted muriatic acid. These calculous concretions were probably the result of diseased action in the vesicular glands, round the orifices of the excretory ducts of which they had been deposited.

The blood.—Upon wounding the animal in almost any part of the body, even in cutting off a portion of the caudal antenna, there oozes out a very clear thin fluid of a bright honey-yellow colour, having sensibly alkaline properties, and coagulating either by heat or by the addition of alcohol. A quantity of this fluid, weighing 1·85 grain, being evaporated under an exhausted receiver, in which was placed dry muriate of lime, left a solid residuum of a bright golden yellow colour, which weighed 0·25 grain: this residuum was brittle, and had the general properties of solid albumen. The foregoing characters render it highly probably that the yellow fluid distributed through the body of the insect resembles in its nature the serum of common blood; and there can be no doubt, arguing physiologically, that this yellow fluid is the blood or nutrient juice of the animal. I wish I could as satisfactorily show the means employed by nature to distribute this fluid through the system of this and other animals of the same class; for, though I cannot hope to discover what more experienced and skilful anatomists have sought in vain, namely, a heart, and a system of circulating vessels; yet I cannot subscribe to their opinion, that the blood transudes through the coats of the intestines, where of course it must be primarily formed, and thence passes, as through the pores of a sponge, to every part of the body. Both Cuvier and M. Marcel de Serres completed a very elaborate set of experiments for the purpose of ascertaining whether the dorsal vessel of insects sends out any lateral branches which might serve the purpose of a circulating system, or whether any other distinct circulating system exists; but they have entirely failed in their endeavours: and I feel assured that where such men have failed, others will not succeed; and yet their consequent supposition, that the blood is diffused through the general substance of the body, appears to me very highly improbable. It accords not with the general character of those means by which nature usually produces its effects; there is too little of art and contrivance, if I may use such terms on such an occasion,

casion, in the mode supposed to be employed. Even in the formation of mineral crystals, which are unorganized bodies, the attraction by which the component particles are aggregated is regulated by laws the most systematically framed and observed: and whoever has viewed with any attention that wonderful monument of human industry and sagacity, the Anatomical Museum of John Hunter, and has there seen the proofs of a sanguineous circulation in animals of an order so low that they can hardly be said to have any specific form or substance, will almost necessarily be disposed to expect a similar provision in a class of animals whose general structure is so elaborately and beautifully organized as that of insects. But I shall again advert to this subject after having described the tracheal system or respiratory organs of the insect under consideration.

The organs of respiration.—As it is very generally known that the atmospherical air, so necessary for the existence of all animated beings, is admitted into the bodies of insects by certain apertures called stigmata, and is then distributed through the system by means of tracheæ or air-tubes, I shall not dwell longer on the description of those organs in the gryllotalpa than is necessary for the elucidation of its particular history.

Omitting the questionable existence of two stigmata in the upper lip, and of two others in the vicinity of the caudal antennæ, there are ten stigmata very distinctly visible on each side of the body*. Hence, therefore, it is necessary to correct (though probably it has ere this been corrected by himself) a statement made by Cuvier in his *Règne Animal*, tom. iii. p. 126, that in the Myriapoda there are twenty stigmata and upwards, but in all other insects eighteen at most. He also asserts in the same place, that insects respire by two principal tracheæ extending longitudinally, one on each side of the body, from which other tracheæ ramify. Now certainly in the gryllotalpa, and, as I have reason to believe, in many other insects also, the longitudinal tracheæ bear so small a proportion in their capacity to the aggregate capacity of the other tracheæ, that in such instances they cannot be called principal tracheæ. My own opinion is, that these longitudinal tracheæ serve as connecting channels, by which the insect is enabled to direct the air to particular parts, for occasional purposes.

Though not immediately bearing on the present point, I beg leave here to state a fact which I have not seen elsewhere noticed; that in the two segments of the body which carry the middle and hind pair of the true legs, in the larvæ of coleop-

* Vide Plate II. fig. 10.

terous and lepidopterous insects, there are no stigmata, discernible at least either to the naked eye, or a common magnifying lens.

But, to return to the stigmata of the gryllotalpa, the first in order, beginning from the head, is situated very near the lower part of the posterior ridge of the thorax. This stigma, not to object to the term in the present instance, is apparently connected with all the tracheæ both of the thorax and of the head itself. It differs remarkably in size and form from all the rest; for instead of being a mere dot or point, it is an elongated fissure, bounded by two horny lips. The second stigma, which somewhat resembles in form, though of much less extent than the preceding, is situated immediately behind the root of the middle leg; the third, which is still less than the second, is situated immediately behind the root of the posterior leg, near the termination of the dorsal part of the third abdominal segment; the fourth, fifth, and onwards to the tenth inclusive, are situated near the terminations of the corresponding dorsal segments of the abdomen.

I would here notice by the way, a peculiar appearance very constantly observable on the ventral surfaces of most of the abdominal segments between the hind pair of legs and the caudal antennæ. At either extremity of those segments there is a short line, not unlike that made by the stroke of a pen, passing obliquely downwards aud inwards: it does not seem easy to conjecture the use of these lines.

I may state from repeated observations, that the stigmata, taken generally, are not the terminations of single tubes: very frequently two, and often more than two, tracheæ originate from the same stigma; and very soon after the commencement, one or even two of these tracheæ subdivide into numerous branches, which follow as nearly as may be the direction of the original tubes.

The distribution of many of the tracheæ may be very satisfactorily demonstrated by drying one of the insects under an exhausted receiver containing muriate of lime: for after having been thus dried the tracheæ become perceptible to the naked eye through the substance of the integuments. The foregoing method of drying anatomical preparations may be successfully employed on many occasions: it answers particularly in the case of the human eye, or the eye of any sufficiently large animal; for, in the act of exhaustion, the air contained in the vitreous humour of the eye becoming expanded, preserves the spherical form of the organ until the whole of the moisture has been evaporated; and it is then sufficiently firm to support itself. I have traced most of the tracheæ to the

parts

parts on which they are respectively distributed; but as no adequate object, nor indeed any object of importance, would be gained by the description of a distribution which is not marked by any physiological peculiarity, I shall only insist on such points as appear to me to be either new or hitherto not sufficiently elucidated.

The tracheæ of insects are generally described as tubes constructed of a spiral thread, the successive coils of which are closely in opposition with each other: such a structure is represented in Swammerdam's plates, and I have no doubt from his acknowledged accuracy that he represents what he observed. It has not however happened to me, with the exception of one equivocal instance, to perceive such a structure in the mole-cricket, the character of the tracheæ of which varies in different parts of the insect; for sometimes they resemble the pulmonary tracheæ of the higher classes of animals, in having an annulated structure; and sometimes they appear as tubes of a perfectly uniform substance like cuticle, or some very thin and unorganized membrane. It is generally understood that the tracheæ of insects penetrate each organ and every part of the body; and certainly the case is such in the instance before us. Thus, in that brush of capillary yellow tubes supposed to constitute the hepatic system, the total number of which amounts to 150 or 200, there is reason to believe that each tube is accompanied by a distinct trachea coiled round it in a long spiral. Again; the two medullary cords which connect the several ganglions of the nervous system, are in their natural state united together by means of the branches of a tracheal tube which runs between them, a similar tube being attached to the exterior edge of the cords; and the surface of what may be called the brain of this insect, is as beautifully characterized by the ramifications of the tracheæ which pervade it, as the surface of the pia mater of the human brain by the blood-vessels which penetrate that membrane in every direction.

In meditating on the difficult problem of the sanguineous circulation of insects, it has forcibly occurred to me that the tracheæ may possibly be the instruments of such a circulation, absorbing the blood or the chyle in the first instance from the internal surface of the alimentary canal, and thence conveying it to the various parts of the body; nor is this opinion, however improbable it may appear, entirely gratuitous. No difficulty, I apprehend, attaches to the supposition that such an absorption may take place, seeing that innumerable minute ramifications of the tracheæ penetrate the intestinal canal in every part; nor does there seem any difficulty in admitting

mitting that the insect may, by the power of exhausting the air from individual tracheæ, draw on the absorbed fluid towards those two lateral tracheal tubes, which are apparently a general medium of communication between all the other tracheæ of the body. And, when once the blood has reached this supposed point of its course, it is manifest that by whatever means the air itself is forwarded from the same point to the most distant parts of the body, by a modification of the same means the blood may be forwarded to the same part; and the elegant proposition of Cuvier, that "the blood being incapable of going in search of the air, the air goes in search of it," will still remain inviolate.

If it should be argued that the tracheæ are not found charged with blood after the death of the animal, it may be answered, that neither are the arteries in the higher orders of animals found charged with blood after their death. However, I have actually seen some of the ramifications of those tracheæ which are connected with the cæca distended with a fluid of the same colour as that found in those organs; and though I have only witnessed this fact in two instances, yet such a fact, even singly taken, must be allowed to be of considerable importance.

Of one thing I am certain, that, after careful observation, I have never found the abdominal viscera, I will not say bathed, as some authors of credit have expressed themselves, in the nutrient fluid which is supposed to have transuded through the coats of the intestines; but I have not even found them lubricated by a greater proportion of moisture than lubricates the intestines of the higher classes of animals.

There is another difficulty which occurs to the hypothesis of the transudation of the chyle through the coats of the intestines; for, if the blood be conveyed to the several parts by previous general diffusion through the interior of the body, and then by absorption into the substance of particular organs, as the hepatic tubes, the *vesiculæ seminales*, and the ovaries; how does it happen that the bile, for instance, does not transude through the coats of the same vessels, the pores of which have admitted the blood from which it has been formed? It may be answered, that the alteration which the blood undergoes in the several organs, changes its properties to such an extent, as to render it incapable of repassing through the pores which admitted it. I cannot of course presume to say that such is not the case; and I am aware that many entomologists will be surprised at, and perhaps disinclined to listen to, the opinion here advanced with respect to a sanguineous circulation in insects; but I nevertheless hope that the opinion will

not

not be rejected without some previous attention to it. With regard to the dorsal vessel of the gryllotalpa, which in this, as in other insects, has been supposed to stand in the place of an arterial heart, I have very few observations to offer. It does not agree in its form with the description commonly given of this mysterious organ; for though it diminishes in diameter as it approaches the head, this is by no means the case towards the other extremity of it. I have not yet completely succeeded in tracing this vessel to its anterior extremity; because as it approaches its termination in that direction, it becomes so delicate as to have hitherto broken under dissection before I arrived at the extremity of it. Towards the opposite extremity it gradually becomes larger from the centre of the body, and terminates apparently in a *cul-de-sac* about the last segment but two of the abdomen.

The muscles.—In the gryllotalpa, as in insects in general, the muscles are exceedingly numerous, and usually very distinctly defined; but as their form and size in different parts of the body may, without difficulty, be conjectured from the form and size of the parts to which they are appropriate, I need not occupy the time of the Society by enumerating or particularly describing them. Those which move the fore legs are remarkable for their size, and apparently fill nearly the whole of the interior of the thorax. Some muscles, as is the case with two belonging to each mandible, and with some of those that are situated within the thigh of the hind leg, have tendons attached to them of considerable extent and strength. I must not omit to mention several parallel muscular bands, which run in a longitudinal direction along the outer coat of the extremity of the great intestine, and are inserted into what may be called the sphincter of the rectum: these muscular bands may evidently assist, by their previous contraction and subsequent relaxation, in discharging that fetid matter which, as has been already said, the animal usually emits when irritated. For the discovery of these muscles I am indebted to Mr. Whessell, whose name I have before mentioned on a similar occasion.

*The nerves**.—In removing the integuments throughout the whole length of the lower surface of the body, we discover a series of nine ganglions, of a pale cream-colour, distributed at unequal intervals from the commencement of the œsophagus to the termination of the rectum; a double medullary cord being continued from one ganglion to another throughout the whole series. The ganglions and their connecting cords lie so nearly

* Vide Plate III. figs. 11 and 12.

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in contact with the common integuments, that great care is requisite, lest in removing these integuments the nerves themselves should be removed, or at least injured. The first of these ganglions, reckoning from the anal extremity of the abdomen, is globular in its form; and is situated between the intestine and the sexual organs, the latter being placed immediately under the ventral integuments. This ganglion gives off several pairs of nerves, of which by far the largest pair may be traced into the caudal antennæ. The second, third, and fourth ganglions are smaller than the first, and are of an oval rather than a globular form: they each send out from two to four or five pairs of nerves. The fifth and sixth ganglions (of which the former is the smallest, the latter the largest ganglion, of the whole series) are situated so closely together, that it is not always easy to demonstrate the connecting medullary cords. The sixth ganglion, which from its size and the number of nerves radiating from it might be called the solar ganglion, is situated between the roots of the posterior legs. The seventh and eighth ganglions are situated respectively between the roots of the middle and the fore legs.

From the eighth ganglion, which lies under the furcular bone of the sternum, two parallel medullary cords pass on to the root of the mandibles, where they unite with the ninth and last ganglion, which is situated under and in contact with the commencement of the œsophagus. This ganglion, which is hollow, as perhaps all the others may be, sends off nerves to the maxilla and adjacent parts: and it sends off besides, two large and important branches which ascending on each side of the œsophagus unite with two corresponding branches that descend from the brain; which organ is situated immediately in contact with the commencement of the œsophagus on its upper surface: so that the œsophagus is placed between the ninth ganglion on its lower surface, and the brain on its upper surface, their connecting branches completing the nervous collar which surrounds it at this part.

The brain differs in colour from the ganglions, being of a pale brownish pink, instead of a cream-colour; and in size it far exceeds the largest of the ganglions. It consists of two hemispheres, separated by a fissure, from each of which pass out four processes. The first of these processes unites, as above described, with a process from the ninth ganglion, to form the nervous collar of the œsophagus; the second passes to the root of the antenna; the third, which may be called the optic nerve, passes towards the inner surface of the cornea, and at its extremity swells out into a fringed coronet of an orange-red colour; the fourth process, the extremity of which is also of an

orange-

orange-red colour, proceeds to the ocellus or stemma of the corresponding side.

The upper surface of the brain is covered by a mass of soft substance somewhat resembling loose fat.

*The sexual organs of the female**.—These organs consist of two ovaries, which occupy a considerable portion of the upper part of the abdomen, and terminate by a narrow duct in a common cavity or uterus, which opens externally under the posterior edge of the last segment but one of the ventral surface of the abdomen. Behind the uterus is an oblong white body, which originating from a *cul-de-sac*, and then doubling on itself in the form of a slender tube, terminates in the uterus. The contents of this body resemble a thin white paste. The ovaries are irregularly pear-shaped, and consist of a transparent membrane irregularly convoluted, through which the ova, enveloped in a gelatinous medium, are easily distinguished. In the same ovary the ova are frequently of different sizes and colours; those which are the largest, and which I suppose to be impregnated, are of a brownish yellow colour: they resist a considerable degree of force before they burst, and the contents when pressed out melt as it were into a soft jelly, leaving a tough membrane which enveloped them. The smaller ova are of various sizes and of nearly a white colour, and of a much more slender and compressed form than those which I have supposed to be impregnated. This difference in the degree of maturation corresponds with a fact stated by Rösel, that the mole-cricket does not deposit all the eggs of the season at one time. In a few instances I found two or three ova which had entered the narrowest part of the duct and were very near the uterus; and from the appearance of these, which may fairly be supposed to be, if not impregnated, at least in a state fit for impregnation, I have ventured to derive the character of the impregnated ovum.

The sexual organs of the male†.—I had dissected several male gryllotalpæ before I was fortunate enough to meet with the sexual organs fully developed; and while I had as yet met with only one animal bearing the character of full developement, I was not certain whether I judged rightly of the natural state of those parts, or whether their uncommon degree of enlargement were not the effect of disease—the disproportion in size between the state in which they had hitherto occurred, and that to which I now allude is so enormous. However, subsequent dissections presenting the same phænomena, I have no scruple in considering them as indicating full developement.

* Vide Plate III. fig. 13. † Ibid. fig. 14.

The testicles of the male are situated similarly to the ovaries of the female, and are not very unlike in general appearance to the ovaries of young females: they differ however in being divided pretty deeply into several unequal lobes, the free extremities of which look towards each other. They send out each a very fine capillary tube or duct; which, descending towards the rectum, is in one part of its passage convoluted on itself so as to resemble the human epididymis partially unravelled.

The excretory duct above described terminates at the bottom of a thick pouch, which is situated between the rectum and the ventral integuments, and in form is not very unlike, though larger than the uterus, opening externally, as the uterus does, under the posterior margin of the last but one of the ventral segments of the abdomen.

The interior mechanism of this pouch is extremely curious; for in the upper part there is contained an apparatus somewhat in the shape of a coronet, of the colour and hardness of tortoise-shell: and at right angles to the centre of this there is fitted a similarly hard and horny substance (in shape resembling a short flat club) which descends towards the external opening of the pouch.

Behind the pouch are situated, one on each side, two oblong white bodies, which are twisted into three spiral coils, and then terminate by an inflected tube at the upper and back part of the pouch. These bodies evidently answer to the *vesiculæ seminales* of insects in general; and resemble in their external character, and in their white pulpy contents, that oval body which is placed at the back of the uterus. There is also another pair of *vesiculæ seminales*, as is frequently the case in insects, situated exteriorly to the former; more slender in form also, and much more convoluted, which apparently terminate near the points where the ducts of the testicles terminate. In the instances of full developement these bodies are enlarged to six times their usual size. Under the circumstances of full developement there is also found, though scarcely perceptible under imperfect developement, a large spherical mass, resembling a ball of eider down, situated immediately at the anterior edge of the pouch above described, and continued on from its substance.

The examination of the mole-cricket has added, as appears from the description of the parts, another exception in the case of the female as well as the male to the general statement, that in insects the sexual organs pass out by the anus. Cuvier mentions, as the only exceptions to this law, the *Iuli* and *Libellulæ*.*

* *Règne Animal*, tom. iii. p. 137

Casting of the skin.—The following are the only observations I have had an opportunity of making as to this point of the history of the mole-cricket. In the process of moulting, the skin of the abdomen appears to split longitudinally down the middle of the upper part; and the skin of the thorax separates in a similar direction; but the skin of the head only separates partially in that direction, and then splits between the stemmata, in a direction towards each of the antennæ; so that the line of separation somewhat resembles the lambdoidal suture of the human skull.

The corneæ of the eyes are cast with the rest of the skin, as in the case of the snake; but they lose their transparency, and become of a grayish white colour.

Even the covering of the claws is cast.

The newly exposed surface of the whole body is covered with the same kind of down as that which covered the preceding skin; except in the case of the long bristly hairs of the caudal antennæ, which apparently are produced afterwards. The colour of the body immediately after the casting of the skin is yellowish white, and it remains of that colour for a few hours: it afterwards gradually darkens.

The organ of sound.—I have very little doubt that the peculiar sound which is characteristic of this insect is produced by the wings; for I have observed in several individuals in their perfect state, that, when irritated, they will separate their upper wings by a brisk motion laterally from each other; and that upon their being suddenly brought back to their natural position, a sound is at the same moment produced, resembling that which I have heard the insect spontaneously produce during the season of summer: but I could not fix the power of producing this sound to either sex exclusively.

There is a peculiar organ, forming a part of the common integuments of the abdomen, and situated between the fourth and fifth stigma on each side; the anterior portion of which consists of a tense membrane, like fine parchment, of a semilunar form: this organ from its individual character might be supposed to contribute towards the production of the sound, but it is found in the female as well as in the male; and its supposed use is not justified by the presence of any internal mechanism.

In two or three instances I have perceived the internal and upper surface of the second abdominal segment, answering to what is generally called the third thoracic segment, furnished with two oblong concave laminæ, terminating in free rounded edges, which are probably elastic; but I feel by no means certain that these are exclusively characteristic of the male, though

I certainly found them most distinctly developed in a male individual.

But my acquaintance with the interesting insect, the history of which has formed the subject of this paper, did not commence till towards the close of that period of the summer during which the animal is heard to produce its peculiar sound: and I propose therefore to resume the investigation of this point at a future opportunity.

Oxford, Nov. 13, 1824.

Dimensions of a full grown Mole-cricket.

	Inches.
Length of the body from the extremity of the lip to the extremity of the vent	2·0
Length of the head	0·165
——————— thoracic division	0·5
——————— abdominal division	1·33
Breadth of the thorax	0·5
——————— abdomen	0·5
Length of the antennæ of the head	0·825
——————— caudal antennæ	0·666
——————— whole alimentary canal	2·0
——————— œsophagus	0·5
Length from the crop to the great intestine	0·5
Length of the great intestine	1·0

Explanation of the Plates II. *and* III.

Fig. 1. Skeleton of the head, viewed from the under side.

Fig. 2. A side view of the animal in its common attitude.

Fig. 3. Skeleton of the thorax.

Fig. 4. Sternum, &c. with the upper part of the thorax adhering.

Fig. 5. Exterior surface of the left fore leg.

Fig. 5[a]. Parasitic insect infesting the roots of the wings; of its natural size, and also enlarged.

Fig. 6. Œsophagus, crop, gizzard, cæca, great intestines, hepatic organ, and anal glands.

Fig. 7. Interior view of gizzard.

Fig. 8. Ditto of a portion of ditto.

Fig. 9. Intestinal worm of the mole-cricket; natural size, and enlarged.

Fig. 9[a]. Upper part of great intestine, with four rows of glands, and the orifice of the hepatic duct.

Fig. 10. The stigmata of the left side; with the organ (situated between the fourth and fifth stigmata) described in p. 414.

Fig. 11. The nine ganglions.

Fig. 12. The brain, surrounding the œsophagus.

Fig. 13. The female sexual organs.

Fig. 14. The male ditto.

LXIV. *On the Theory of the Figure of the Planets contained in the Third Book of the* Mécanique Céleste. *By* J. Ivory, *Esq. M.A. F.R.S.*

NO part of the philosophy of Newton has advanced more slowly than that which treats of the figure of the planets. This speculation, depending more immediately upon the principle of a mutual attraction between all the particles of matter, was entirely new, and required every thing to be created. Much is done in the *Principia*, the greatest monument of human genius that has yet appeared; and much has been added in many researches by all the great mathematicians who have laboured to fill up the outlines traced with such inimitable skill in that illustrious work. It is not the present intention to enter upon any detail of the steps by which this branch of philosophy has advanced to its present state; it will be sufficient to remark that all our knowledge on this subject naturally falls under two different heads. The first of these comprehends the discoveries of Maclaurin, with which I arrange all that is contained in the second part of Clairaut's excellent treatise on the Figure of the Earth. In the view of the subject taken under this head, it is not proposed to investigate *à priori* the figures of equilibrium; it is merely demonstrated that the elliptical spheroid of revolution will fulfill those conditions. Newton was under the necessity of making some gratuitous assumptions in order to evade the great difficulties that occurred in treating of the figure of the earth. He supposed that it was composed of a homogeneous fluid, and that it had the form of an elliptical spheroid of small oblateness at the poles; and upon these suppositions he investigated the degree of the oblateness, or the proportion between the equatorial and the polar diameters. The researches both of Maclaurin and Clairaut were undertaken for the purpose of inquiring into the legitimacy of the assumptions made by Newton. They both succeeded in demonstrating the truth of the suppositions on which the investigation in the *Principia* is founded. Maclaurin's demonstration, in which the method of the ancient geometry is followed, is very elegant and accurate, and is at this day the only case of the problem that has been solved in a manner strictly rigorous and without having recourse to approximations. Clairaut used the modern analysis; and admitting the supposition of elliptical spheroids of small oblateness which allowed the square of the excentricity to be neglected, he extended his researches to the case when the earth is composed of fluid strata varying in density from the centre

centre to the surface, and likewise to the case when it consists of a solid nucleus covered with one or more fluids.

The labours of Maclaurin and Clairaut are here classed together, because they are confined to investigating certain properties of elliptical spheroids. But why is it necessary to suppose a particular figure? Ought not the figure of a planet, if we suppose it fluid, to be deduced from the general laws of equilibrium? What is it that makes the investigation succeed when an elliptical spheroid is supposed, thus seeming to exclude every other figure? Is there but one figure of equilibrium? And, if this be the case, how is the demonstration to be made out?

The second division of our knowledge respecting the figure of the planets is likewise the development of the general view of the problem proposed by Newton. A fluid body, whether homogeneous or composed of strata of variable density, if it be at rest and subjected only to the attraction of its particles, cannot be *in equilibrio*, unless it have the figure of a sphere. If the sphere begin to revolve about an axis, the fluid will subside at the poles, and become protuberant at the equator; and the question is to investigate the nature of the new figure according to the laws of equilibrium. There are two points that must be previously discussed, in order to ensure success in this research. We must be able to estimate the variation of the attractive force produced by the change of figure; or, which is the same thing, a method is required for computing the attraction of a body that is little different in its figure from a sphere. We must likewise know the conditions necessary to the equilibrium of a fluid body, the particles of which are acted upon by any forces. In the Memoirs of the Academy of Sciences for 1784, Legendre first gave a method for the attraction of spheroids little different from spheres in a form fit to be employed in the investigation of their figures of equilibrium. He deduced from his analysis that the earth, supposing it homogeneous and to be a figure of revolution, must have its meridians elliptical in order to fulfill the conditions of equilibrium; which is in fact to solve the problem as conceived by Newton without making any gratuitous assumption. Laplace generalized and improved the method of Legendre, adapting it to the supposition of a variable density, and to the case of a fluid covering a solid nucleus. In his hands it has become an extensive branch of analysis, and is made the foundation of the theory of the figure of the planets contained in the third book of the *Mécanique Céleste*.

With regard to the equilibrium of a fluid body, Newton employed the principle of the balancing of the columns reaching

ing from the centre to the surface; Huygens proposed the perpendicularity of gravity to the outer surface. These are properties which undoubtedly both belong to every fluid body *in equilibrio;* and it deserves to be remarked, that either of them is sufficient for the equilibrium of a homogeneous fluid if we suppose that it has the figure of an elliptical spheroid. But it was soon found that there are some figures which would be *in equilibrio* by the one principle, and not so according to the other. Bouguer therefore imagined that the two principles must both concur in every case of true equilibrium. Clairaut afterwards showed that, even with this restriction, we are not sure that an equilibrium will always take place. Abandoning all precarious suppositions, that able geometer, in the first part of his work on the Figure of the Earth, has deduced a more satisfactory theory from the laws of hydrostatics, which, without any change in its principles at least, has ever since been universally adopted. It is however extremely remarkable that, in the second part of his work, Clairaut has not been able to investigate the figure of the earth from his theory of equilibrium, but is obliged to adopt the supposition of an elliptical spheroid. It may be said, indeed, that in his time mathematical knowledge was not sufficiently advanced to overcome the difficulties of so intricate a problem. But it is still true at the present day, that, what Maclaurin rigorously proved by means of the synthetic geometry of the ancients, has never been deduced, except by approximation and in a very particular case, from the modern theory of the equilibrium of fluids. We may add, that the deduction not only has not been made, but that it never can be accomplished; because there is wanting a principle necessary for determining the figure of equilibrium. Clairaut's theory of fluids is in the same predicament with the principles advanced by Newton and Huygens; though just and accurate, it is incomplete, at least in the question of the figure of the planets.

The theory in the *Mécanique Céleste* applies only to spheroids little different from spheres. It is founded on the equilibrium of fluids usually received. The results obtained, at least in a first approximation, are exact; for they agree with what has been proved with undoubted evidence by Maclaurin and Clairaut. But as the theory of fluids on which the investigation proceeds is in general insufficient to determine the true figures of equilibrium, it follows that there must be some peculiarity in the case considered, something hidden probably under the analytical operations employed, which corrects the general defect, and leads to a true result in particular circumstances. In every physical investigation, it is of great importance

tance to distinguish between the foundations of our reasoning and the mathematical processes, of which the office is not to supply, or stand in the room of, principles, but to deduce the consequences that flow from them. In the present instance, although we admit the first approximation to be true, yet, unless the grounds of the method are fully cleared up, we must remain doubtful with regard to the succeeding steps in which we attempt, by reiterated operations, to approximate more and more to the exact figure of equilibrium.

At the present time, when so much has been done, and is still doing, to determine the figure of the earth experimentally, it seems proper likewise to reconsider the theory. With regard to the writings of Maclaurin and Clairaut, no examination is required. If these authors have taken a less extensive view of the problem, the grounds and the manner of their reasoning need no elucidation. But it appears from what has been said, that the method delivered in the *Mécanique Céleste* for the equilibrium of spheroids little different from spheres, is not entirely free from objection in its physical principles; and the illustrious author has occasioned some difficulties by the manner in which he has laid down the fundamental proofs of the analysis he employs. I propose therefore to make some observations, first on the analysis, and secondly on the physical principles of the theory of the figure of the planets contained in the third book of the *Mécanique Céleste*. In very intricate cases it appears to be the destined lot of humanity to approach the truth very slowly, and by reiterated efforts. Difficulties arise and are overcome in succession; and this progressive improvement is, perhaps, no where more strongly exemplified than in the branch of philosophy to which our attention is at present directed. The theory on which I propose to remark has been before the public for more than a quarter of a century; it has great merit, and has been highly applauded; and if we now presume to make it the subject of examination there can be but one apology, which is, the justness and the truth of the strictures to be made.

I. Conceive a spheroid very little different from a sphere; and having assumed any point upon its surface, let a straight line r be drawn inwards, at right angles to the surface, so as to reach very near the middle or centre. From the extremity of r describe a sphere having for its radius a line a nearly equal to, but less than, r; then it is plain that any radius of the spheroid may be represented by the expression $a(1 + \alpha y')$, α being a small coefficient, of which the square is to be neglected, and y' a function of two variable arcs that determine the position of the radius. We shall denote the arcs here mentioned

mentioned by θ' and ϖ'; θ' being the distance on the sphere's surface, between the variable radius and a fixed axis or diameter, and ϖ' the angle contained between θ' and a determinate great circle passing through the same axis. When the variable radius coincides with the line r, we shall suppose that θ', ϖ' and y' have the particular values θ, ϖ and y; in consequence of which there will result $r = a\,(1 + \alpha y)$.

Instead of referring the variable radius to an axis assumed arbitrarily, we may ascertain its position with respect to the line r. Let ψ denote the arc between the variable radius and the line r, and ϕ the angle contained between ψ and the former arc θ drawn between r and the fixed axis: then ψ and ϕ will determine the position of the variable radius, and y' will be a function of ψ and ϕ, as well as of θ' and ϖ'. The arcs θ, θ', ψ are the three sides of a spherical triangle on the surface of the sphere, and the angles opposite to θ' and ψ are ϕ and $\varpi - \varpi'$. Hence we obtain, by the rules of spherical trigonometry,

$$\cos\theta' = \cos\theta\cos\psi + \sin\theta\sin\psi\cos\phi,$$
$$\sin\theta'\sin(\varpi - \varpi') = \sin\psi\sin\phi,$$
$$\sin\theta'\cos(\varpi - \varpi') = \sin\theta\cos\psi - \cos\theta\sin\psi\cos\phi;$$

and from these expressions it readily follows that the three rectangular co-ordinates,

$$\cos\theta',\ \sin\theta'\cos\varpi',\ \sin\theta'\sin\varpi',$$

are linear functions of the three,

$$\cos\psi,\ \sin\psi\cos\phi,\ \sin\psi\sin\phi.$$

It appears, therefore, that if y' be a rational and integral function of the first three co-ordinates, it may be transformed into a like function of the other three.

We now proceed to the fundamental demonstration of Laplace's method; namely, that of the equation which takes place at the surface of the spheroid. Let f denote the distance of the point assumed on the surface from any molecule dm of the spheroid; then, supposing the attractive force to be as the nth power of the distance, let

$$V = \int f^{n+1}\,dm$$

the integral being extended to the whole mass of the spheroid; and V will be the function which, by its differentiation, gives the attractive force of the spheroid upon the assumed point in any required direction*. Now V consists of two parts. The first part is relative to the sphere of which a is the radius; it is evidently a function of r, which we shall denote by A. The second part is relative to the stratum of matter between the surfaces of the sphere and the spheroid; and, on

* *Mécanique Céleste*, livre 3^{me}, § 10.

account of its thinness, it may be considered as a series of molecules spread over the surface of the sphere. Conceive the surface of the sphere to be divided into an infinite number of elementary parts, or differentials, as ds; the position of ds being determined by the arcs θ' and ϖ', or ψ and ϕ: then the distance of ds from the assumed point will be equal to $\sqrt{r^2 - 2ra\cos\psi + a^2}$; and the thickness of the molecule standing upon ds being $\alpha a y'$, the part of V relative to the whole stratum will be equal to

$$\int (r^2 - 2ra\cos\psi + a^2)^{\frac{n+1}{2}} . \alpha a y' . ds,$$

the integral being extended to the whole surface of the sphere. If now we use f to denote $\sqrt{r^2 - 2ra\cos\psi + a^2}$, and add together the two parts of V, we shall get

$$V = A + \int f^{n+1} . \alpha a y' . ds.$$

Differentiate this equation with respect to r, then

$$r\frac{dV}{dr} = r\frac{dA}{dr} + (n+1)\int (r^2 - ra\cos\psi) f^{n-1} . \alpha a y' . ds.$$

But, $r^2 - ra\cos\psi = \frac{1}{2}f^2 + \frac{1}{2}(r^2 - a^2)$; wherefore

$$r\frac{dV}{dr} = r\frac{dA}{dr} + \frac{n+1}{2}\int f^{n+1} . \alpha a y' . ds + \frac{n+1}{2}(r^2 - a^2)\int f^{n-1} . \alpha a y' . ds.$$

If now we combine this expression with the value of V, we shall readily obtain

$$\frac{n+1}{2}V - r\frac{dV}{dr} = \frac{n+1}{2}A - r\frac{dA}{dr} - \frac{n+1}{2}(r^2 - a^2)\int f^{n-1} . \alpha a y' . ds.$$

Here the last term on the right hand has the factor $r^2 - a^2$, which is very small, because a is nearly equal to r: and if we suppose that a increases till the sphere touches the spheroid, the factor will be rigorously evanescent, and the term multiplied by it will, generally speaking, likewise vanish. Now if we observe that when a increases to be equal to r, the thickness $\alpha a y'$ becomes $\alpha a (y' - y)$ the last equation may be separated into these two formulæ, which, generally speaking, are true when $r = a$, viz.

$$\left.\begin{aligned} \frac{n+1}{2}V - a\frac{dV}{dr} &= \frac{n+1}{2}A - a\frac{dA}{dr} \\ (r^2 - a^2)\int f^{n-1}(y' - y)\, ds &= 0 \end{aligned}\right\} \quad (1)$$

The first of these formulæ is Laplace's equation at the surface of the spheroid; and it is true in every case when the integral in the second, after being multiplied by the evanescent factor $r^2 - a^2$, makes a product equal to zero. Having now brought the matter under consideration to a form proper for

discussion, it may be observed here that, in what follows, the value of the integral is not estimated by the ordinary rules. The reasoning turns on this principle: that whatever is proved of every individual element or differential must be true of the aggregate or integral. No principle certainly can be more clear or less exceptionable; and all that we have to do is to apply it strictly, and to assure ourselves that in every case the property to be proved does really belong to every individual element without exception.

In the first place, when the exponent n is positive and greater than 1, the equation at the surface is true in the most extensive sense: for every element of the integral in the second formula being finite, any aggregate of them will be equal to nothing on account of the evanescent factor.

We come next to the case when n is negative. If we write $-n$ for n, the second formula (1) will become

$$\int \frac{(r^2 - a^2)\,(y' - y)\,ds}{f^{n+1}};$$

and here we see that when $\cos\psi = 1$, and $f = r - a$, the element of the integral may, on account of the denominator, become infinitely great, notwithstanding the evanescent factor in the numerator. But in order to investigate this case with clearness, there are some considerations to be attended to.

It is to be observed that $y' - y = 0$ when $\cos\psi = 1$. As the molecule approaches the point of contact of the sphere and spheroid, we may suppose that its thickness decreases in the same proportion with f^2, the square of its distance from that point. It appears from the explanation which the author has given in his later writings *, that this circumstance is to be understood in the demonstration in the *Mécanique Céleste;* for it is not expressly mentioned, and no stress is laid upon it. It was the more necessary to be explicit on this head, because the property assumed may not belong to every function that y' may be supposed to stand for. It seems an omission not to distinguish the cases that come under the demonstration from those to which it will not apply. It is curious too that this point escaped the penetration of Lagrange, whom it would have helped to clear up that analytical mystery which he calls a paradox in the integral calculus †.

Another thing to be observed is, that $y' - y$ being considered as a function of ψ and φ, the element of the surface of the sphere ds will be equal to $a^2\,d\psi \sin\psi\,d\varphi$; whence it follows that near the point of contact ds will decrease in the

* *Mécanique Céleste,* livre 11^me^, chap. 2.

† *Journ. de l'Ecol. Polyt.* tom. 8.

same proportion with f, and the quotient of the former, divided by the latter, will tend to a finite limit.

These things being premised, the foregoing integral may be thus written,

$$\int \frac{r^2 - a^2}{f^{n-2}} \times \frac{y' - y}{f^2} \times \frac{ds}{f}:$$

and according to what has been said, the two factors on the right hand have always a finite value; but when $\cos \psi = 1$, and $f = r - a$, the remaining factor, instead of being evanescent, is finite when $n = 3$, and infinitely great when n is greater than 3. In both cases the demonstration fails: in the one, at least some further discussion is necessary; and in the other, the element of the integral, and consequently the integral itself, are both infinitely great, instead of being evanescent. The equation at the surface of the spheroid must therefore be understood with some restriction when the attractive force is supposed to be proportional to any power of the distance. It is true for all positive powers; but it holds not when the exponent of the power is negative and greater than 3, at least when the stratum of matter is spread over the whole surface of the sphere. In his later writings the author makes no mention of the general demonstration he had given in the *Mécanique Céleste;* he confines his views to the law of attraction that obtains in nature, which is certainly the most important case of all, as it is the only useful one. We have next to examine this case particularly.

In the case of nature, when the attraction is inversely proportional to the square of the distance, the quantity A is the sum of the molecules of the sphere divided by their distances from a point without the surface; it is therefore equal to $\frac{4\pi a^3}{r}$; and hence, $r \frac{dA}{dr} = -\frac{4\pi a^3}{3r}$: wherefore, since $n = -2$, the formulæ (1) will become

$$\left.\begin{aligned} \tfrac{1}{2} V + a \frac{dV}{dr} &= -\frac{2\pi a^2}{3} \\ \int (r^2 - a^2) \times \frac{(y' - y)\, ds}{f^3} &= 0 \end{aligned}\right\} \qquad (2)$$

Now, admitting that $y' - y$ decreases near the point of contact in the same proportion with f^2,* it is evident that the second formula will be always equal to zero, on account of the evanescent factor. The equation at the surface of the spheroid is therefore, on this hypothesis, strictly demonstrated, and is true whether the stratum of matter is spread over the

* *Mécanique Céleste*, livre 11me.

whole

whole surface of the sphere, or covers it in any manner partially. It remains to ascertain in what cases the supposition assumed is true, an inquiry totally omitted by the author of the demonstration. It must be recollected that y' is a function of the arcs θ' and ϖ'; that y is the value of y' when θ' and ϖ' have the particular values θ and ϖ; and that ψ is the arc on the surface of the sphere between y' and y. When $r = a$, then $f^2 = 2a^2(1 - \cos\psi)$; and the supposition on which the demonstration proceeds is evidently that $\frac{y' - y}{1 - \cos\psi}$ must have a finite value when the numerator and the denominator are both evanescent. We want to know to what class of functions this property belongs; for to such only the demonstration will apply. If it were asserted that the demonstration is exact, whatever function y' stands for, it would be sufficient for destroying it, to show that it fails in one particular instance. But, in order to avoid minute discussion, we may remark that the analysis of Laplace is solely employed about rational and integral functions of three rectangular co-ordinates of a point in the surface of a sphere; and we shall prove that such expressions do not come under the foregoing demonstration.—Now if y' be a function of the co-ordinates, $\cos\theta'$, $\sin\theta'\cos\varpi'$, $\sin\theta'\sin\varpi'$, it may, as already noticed, be transformed into a similar function of the co-ordinates $\cos\psi$, $\sin\psi\cos\varphi$, $\sin\psi\sin\varphi$: and again, by substituting for the powers and products of $\sin\varphi$ and $\cos\varphi$, their values in the sines and cosines of the multiples of the arcs, the latter function will take this form; viz.

$$\mathrm{H}^{(0)} + \mathrm{H}^{(1)}\sin\psi\cos\varphi + \&c.$$
$$+ \mathrm{K}^{(1)}\sin\psi\sin\varphi + \&c.$$

the symbols $\mathrm{H}^{(0)}$, $\mathrm{H}^{(1)}$, $\mathrm{K}^{(1)}$, standing for rational and integral functions of $\cos\psi$. Wherefore $y' - y$ will be equal to

$$(\mathrm{H}^{(0)} - y) + \mathrm{H}^{(1)}\sin\psi\cos\varphi + \&c.$$
$$+ \mathrm{K}^{(1)}\sin\psi\sin\varphi + \&c.$$

In this expression all the terms vanish separately when $\psi = 0$; whence it follows that $\mathrm{H}^{(0)} - y$ is divisible by $1 - \cos\psi$. But the two next terms, multiplied by $\cos\varphi$ and $\sin\varphi$, are not so divisible, the quotients in both cases being infinitely great, instead of being evanescent, when $\cos\psi = 1$. Wherefore the quantity $\frac{y' - y}{1 - \cos\psi}$ does not tend to a finite limit, but is ultimately infinitely great; and the demonstration entirely fails in the particular case considered, which is in reality the only one for which it is wanted.

We know however that, when y' is a rational and integral function of three rectangular co-ordinates, the equation at the surface,

surface, and all the other properties that occur in the analysis of Laplace, are strictly true, because they may all be demonstrated by the ordinary rules. If this should occasion any difficulty, it is easily removed. In place of the element of the surface of the sphere $d\,s$, substitute its value $a^2\,d\,\psi \sin\psi\,d\,\varphi$; then, observing that the arcs ψ and φ are independent of one another, the second of the formulæ (2) may be thus written,

$$\int (r^2 - a^2) + \frac{a^2\,d\,\psi \sin\psi \int (y' - y)\,d\,\varphi}{f^3} = 0:$$

and the integral $\int (y' - y)\,d\,\varphi$, taken between the limits $\varphi = 0$ and $\varphi = 2\,\pi$, will be reduced to $2\,\pi\,(\mathrm{H}^{(0)} - y)$. The refractory terms have now disappeared; and the investigation may be completed either by the method proposed by Laplace, or by the ordinary rules. Thus in whatever manner we view this analysis, if we push our reasoning till the clouds of obscurity are completely dispelled, we are uniformly brought to one conclusion, namely, that it must be restricted to rational and integral functions of three rectangular co-ordinates.

If, laying aside analytical symbols and operations, we wish to contemplate the real grounds of the method in the nature of the things concerned, we shall find that all the difficulties arise from the author's considering as differentials the portions of the stratum that stand upon the elements of the spherical surface, which is allowable only when the small masses of matter are at a great distance from the point of contact. If I wish to estimate the relative attraction of the mountain Schehallien on a point at a great distance on the surface of the ocean, it may be sufficient to divide the mass of the mountain by the square of the distance. But if the attracted point be close to the mountain, as in Dr. Maskelyne's experiment, the former method would be entirely erroneous: we must now divide the mountain itself into differentials, and sum up the attractive forces by the rules of the integral calculus. The equation at the surface of the spheroid is therefore liable to limitations, which depend upon the law of the attractive force.

Most of the foregoing observations are to be found in a paper presented to the Royal Society in 1811*. Since that time there has been no question about the general demonstration in the *Mécanique Céleste* for an attractive force proportional to any power of the distance; but, in the case of nature, or when the attraction is inversely proportional to the square of the distance, some attempts have been made to prove the equation at the surface of the spheroid without specifying particularly the function that expresses the thickness of the

* Phil. Trans. for 1812.

stratum.

stratum. I have here examined that which the author of the *Mécanique Céleste* published in the *Mém. Acad. des Scienc.* 1818, and afterward, in 1823, in the 11th book of the *Mécanique Céleste;* and I have shown its insufficiency to serve as the groundwork of the theory of the figure of the planets. This sort of analysis, and even the method of demonstrating which the author has chosen to adopt, have acquired some celebrity, and have been applied in various researches; and it seems to be of some importance to the progress of mathematical knowledge, that the grounds of it, and the limitations to which it is subject, be well understood. For this reason I shall, on another occasion, offer some additional remarks on this point before I proceed to the second branch of my subject.

Nov. 30, 1825. JAMES IVORY.

[To be continued.]

LXV. *Report of the Transactions of the Academy of Natural Sciences of Philadelphia during the Year* 1824; *submitted by the Recording Secretary, in pursuance of a Resolution of the Academy.*

[Concluded from p. 354.]

§ 2. *Geology and Organic Remains.*

THE intimate relation which exists between the study of the structure of the earth, and that of the remains of a former world which are seen in many of its strata, requires that the examination of fossil remains, whether animal or vegetable, should be always kept connected with the observations upon the mineral composition of our globe itself.

Seven communications were received in this department of science. The first, which is entitled "Description of a testaceous formation at Anastasia Island, &c. by R. Dietz," describes a very singular rock, of which Mr. Dietz brought home many specimens last year, and which had attracted the attention of our geologists, being almost entirely composed of fragments of various shells agglutinated together. Mr. Dietz states that Anastasia Island, situated opposite to St. Augustin, along the coast of East Florida, has a considerable portion of its northern, and perhaps the substratum of the remaining part of the island, formed entirely of this interesting aggregate, in which Mr. Say has recognised fourteen different species of shells belonging to the genera *Arca*, *Mactra*, *Donax*, *Crepidula*, *Lucina*, *Natica*, *Oliva*, *Nassa;* the three first constituting nearly the whole mass of the rock,—of each of the other genera one individual alone having been observed. Mr. Dietz offers a theory

theory of the formation of this rock, which will be read with interest by all those who are disposed to extend the science of geology beyond the mere observation of facts; and to include in it speculations tending to explain under what circumstances the various rocks were formed.

Dr. Harlan has read to the Academy five communications, in which he describes several organic remains of considerable interest. The first of these was a fragment found by Lewis and Clarke near Soldier's River, a tributary of the Mississippi. This bone, which belongs to the valuable collection of the American Philosophical Society, will constitute a new genus among the Saurian *reptilia:* it differs from the *Ichthyosaurus* and the other genera to which it is allied, by the circumstance of the bodies of the teeth being in close contact, and by the absence of the perforation which they present through the body of the bone, offering a canal for the passage of the inferior maxillary nerve. In Dr. Harlan's specimen there is a groove running the whole length of the dental bone, immediately beneath the alveolar portion on the mesial aspect of the bone; from all which considerations, Dr. Harlan forms a new fossil genus, under the name of *Saurocephalus;* and from the lanciform nature of its teeth, distinguishes this species by the name of *Saurocephalus lanciformis.*

By a reference to the collection of British fossils in the Philadelphia Museum, Dr. Harlan observed in it a specimen which presents specific characters that have appeared to him sufficient to distinguish it from those described by the Rev. William D. Conybeare and other writers. He therefore describes it as a new species, under the name of *Ichthyosaurus coniformis.*

The many organic remains which exist in the tertiary formations of New Jersey have attracted this author's attention, and have given rise to a memoir "On an extinct species of crocodile not before noticed, and some observations on the geology of West Jersey." This paper contains some interesting remarks upon what has been usually considered as a marl, but which the author is led to rank as a ferruginous clay, without any claim to the title of marl. In this clay many fossil *reliquiæ* have been found, such as *Terebratula*, *Ostrea*, *Belemnites*, *Ammonites*, &c. also bones or teeth of sharks, crocodiles, turtles, and a very remarkable tooth belonging to some Ichthyosaurian reptile, &c. In another communication upon the same formation, the author notices three vertebræ belonging to some Saurian reptile unlike any hitherto described, and the type of which is not known to have existed in North America. One of these was sufficiently well preserved to enable Dr. Harlan

to

to ascertain that while it belonged to the genus *Plesiosaurus*, it must, from its magnitude and the proportion of its parts, have constituted a distinct species. This paper contains also some observations on the manner in which the ribs of the different genera of the Saurian family are articulated. He likewise notices the teeth of several species of sharks: of these teeth, two, belonging to the *Squalus carcharias*, measure five inches in length, and four in breadth at their base; and, allowing that the fossil animals were proportioned like the recent ones, indicate that the individuals to which they belonged must have exceeded forty feet in length.

But the most extensive communication in this division of science is "Mr. Say's account of some of the fossil shells of Maryland," which were collected by Mr. Finch, who expects to describe the geological circumstances under which they are found. Mr. Say's descriptions include about forty new species, besides a notice of several others previously described by himself and other authors.

Every thing which connects itself with the history of that large animal which at one time ranged over our country, and whose bones have been found in many very distant places, is of such general interest, that the "Description of the *Os hyoides* of the Mastodon," by Dr. Godman, cannot fail to give gratification to all who are anxious to see the structure of this gigantic animal fully illustrated. The description of the bone is full, and acquires some additional interest from the fact that "the whole of the basis, appendix and cornu, have not been fossilized, but still retain the characters of bone."

§ 3. *Mineralogy.*

Dr. Troost is the principal contributor to this department, having read to the Academy four papers, three of which have been published, and the fourth will probably soon be so. This author's first communication refers to the petalite, a mineral which had not yet been found in America, but the existence of which was recognised by him in some specimens brought from Lake Ontario by Dr. Bigsby. The presence of lithia in these specimens confirmed their connexion with the European petalite.

In a second communication Dr. Troost describes, under the name of *Unibinaire*, a new form of chrysoberyl, which he observed in a specimen from Saratoga, in the state of New York, where it is found in a Pegmatite. The *unibinaire* "has the appearance of a short hexahedral prism, of which two of the six edges are bevelled."

Dr. Troost's third communication refers to a new crystal-

line form of the andalusite. It occurs in a specimen from Litchfield in Connecticut. The *progressive*, as our author terms this variety, is described as a six-sided prism, having four emarginated edges terminated by a dihedral summit.

Dr. Troost's last memoir, which is still in the hands of the Committee of Examination, contains observations on the minerals discovered at Franklin, New Jersey.

Mr. Bowen has furnished the Academy with two communications, one of which is an analysis of a copper ore, which appears to be a bisilicate of copper with water, and which occurs at Somerville in New Jersey. Mr. Bowen's analysis proves the absence of phosphoric acid, which had been supposed to be one of the ingredients of this mineral.

A mineral which was discovered by Dr. M'Clellan at Saybrook, in Connecticut, in 1817, has been examined by Mr. Bowen, and proved to be new: he has dedicated it to Professor Silliman. It bears some analogy to anthophyllite, and was described under that name in Cleaveland's second edition. It occurs in rhomboidal prisms, and has but one cleavage parallel to the longer diagonal. Its analysis proves it to be a silicate of alumine, with an accidental portion of oxide of iron.

The experiments which Professor Silliman has made on charcoal are well known, and have excited much interest, and some division of opinion, both abroad and at home. They have been repeated by several chemists, and among others by Dr. Macneven of New York, who obtained, by means of Dr. Hare's deflagrator, a globule, which has since been examined by Mr. Vanuxem, who has communicated his results to the Academy. After an analysis of the same, Professor Vanuxem concludes "that this product of the fusion of charcoal must consist merely of the impurities contained in the charcoal, and is not a fusion of its carbon as has been supposed."

This communication led to a reply from Dr. Hare, whose object was to prove that the substance examined by Mr. Vanuxem differed materially from the products obtained by Mr. Silliman, and that the conclusions drawn from the former were inapplicable to the latter. In this state the question now stands; and we shall forbear offering any remarks, as the Academy has appointed a committee of its members to repeat the experiments, and ascertain the facts*.

Finally, a communication has been received from Messrs. Vanuxem and Keating, containing some observations on the minerals discovered at Franklin, New Jersey. In this paper

* See Phil. Mag. vol. lxiv. 467, lxv. 283, lxvi. 161 and 455.

the

the authors retract the opinion which they had advanced concerning the existence of the Jeffersonite as a distinct species, the crystals which have been found since having shown its analogy to pyroxene. This paper contains some additional observations on the Franklinite, the red zinc ore, the carbonate and silicate of zinc.

Most of the memoirs, of which we have thus attempted to offer an abstract, have been published. These, together with two papers read the preceding year, and which had not been then published, have furnished abundant matter for the Journal of the Academy, which has been conducted this year with more activity than at any preceding period. The papers to which we have alluded, are Mr. Say's "Description of the Coleopterous Insects of the United States," and a "Notice of the discovery of spodumen, by Mr. Nuttall." The Academy has published this year two half-volumes; and a number of communications remain on hand, which will be put to press early in the year 1825.

There is, however, a department of science which appears to be somewhat neglected among us at present; this is Botany. The valuable communications which the Journal formerly received on this subject from our eminent associate Mr. Thomas Nuttall, have been interrupted by his removal from this city.

This extensive communication of scientific investigations has, of course, rendered the meetings of the Academy very attractive to its members, and even to strangers. The attendance of the members has been greater during the closing year that at any preceding period, and evinces a growing taste for science in the community. This may also be attributed in part to the system of lecturing, which has been pursued with considerable regularity during the present year. Although the lectures are frequently on elementary or general subjects, yet they are of use, by enlarging the views of those who have already acquired proficiency in one department, or by interesting such as have not made a regular study of any particular branch of science.

Our list of members has increased by the accession of ten new associates, while three only have tendered their resignation. The Academy has incurred no loss by death. The correspondents elected this year amount to five; one has resigned. Your secretary is not aware of any death among the correspondents, though it is probable that some have taken place.

Your foreign correspondence has been greater this year than at any former period. It at present includes some of the most distinguished naturalists of Scotland, England, France, Ger-

many and Italy, and promises to become of very great importance.

The additions to your library and museum have been very great; and among its benefactors, the Academy has still to acknowledge with gratitude that its president, Mr. Maclure, stands foremost. His unequalled munificence to this institution suffers him to lose no opportunity which his protracted visited to Europe offers him, to enrich your library and collections.

The judicious measure adopted this year, by which the curators are authorized to exchange the duplicates, will, by removing useless specimens and substituting new ones, lead to the completion of the museum.

Finally, the finances of the Academy are in an improving condition. The report of the auditors, made this evening, will show that the heavy debt, which has so long cramped our efforts, has been at last extinguished, and that there remains a small balance for investment, which, if judiciously stored up, may become the foundation of a fund that will enable the Academy to extend its usefulness.

While our affairs are thus in a flourishing condition, we may be permitted to feel some satisfaction at the success which has attended our efforts, and to indulge the hope that, by persevering during the next year in the same course, we may more extensively promote the interests of the noble cause in which we are all so deeply interested.

All which is respectfully submitted by

WILLIAM H. KEATING,
Recording Secretary.

Philadelphia, Dec. 28, 1824.

APPENDIX.—*List of Lectures delivered before the Academy of Natural Sciences during the year ending December* 28, 1824.

1. Proofs that the brain is the organ of the mind. 2. On the power of animals to retain their internal natural temperature. 3. On the substances that enter into the composition of animal bodies.—By Dr. Coates.

4. On the dilatation and contraction of the heart. 5. Review of White's gradation of animals. 6. Same subject continued.—By Dr. Darrach.

7. On comets, with a new theory to account for their nature, &c.—By Mr. Gilpin.

8. On some late anatomical investigations. 9. On the anatomical structure of the eye.—By Dr. Godman.

10. On the influence of climate upon the geographical distribution of plants. 11. On classification in botany.—By Dr. Griffith.

12. On

12. On the physiology of the brain. 13. On the physical characters which distinguish man from the anthropomorphous animals.—By Dr. Harlan.

14. On the formation of dew, with an examination of the different theories which have been offered to account for it. 15. On climate.—By Dr. Hays.

16. On colour as a character in minerals, and on the limited degree of importance to which it is entitled. 17. On the opinions entertained by the Wernerians respecting the modifications of the surface of the earth by fire.—By Mr. Keating.

18. On a north-west passage. 19. On hybernation.—By Mr. Lea.

20. On the circulation of the blood. 21. On respiration. —By Dr. Mitchill.

22. On comets, with a new theory to account for their tails. —By Dr. Patterson.

23. Introduction to the study of entomology.—By Mr. Say.

24. On the rocks which enter into the composition of the globe. 25. On the distribution of rocks into formations.— By Dr. Troost.

LXVI. *On the unequal Evolution of Heat in the Prismatic Spectrum.* *By Dr.* T. J. Seebeck.

[Concluded from p. 343.]

WHAT influence will a change in the temperature of the air have on these phænomena? What is the proportion of the differences of the heat in the spectrum in warm and cold days? It is easily to be supposed that in cold days the heat in the spectrum will be generally found less, as the air more quickly cools the thermometer, as it is warmed by the light, than on a warm day; which difference will be expressed by smaller figures. The proportion of the degrees of heat in the different colours of the spectrum remains however the same, the experiment being made on a cold or warm day, provided all other conditions are the same,—the days being equally clear, the prisms being the same, the positions normal, the distance of the thermometer and the opening in the shutter or in the prism being also similar. If then the blue and violet halves, for example, are compared with the red and yellow, we shall always obtain the same differences.

From all the observations and experiments here given, it results that the effects are variously altered by the refracting mediums being of different descriptions, and no less by external influences.

Thus

Thus the difference in the statements of the philosophers mentioned at the beginning of this essay, respecting the place of the greatest heat, will also be more easily understood. Those who did not make use of similar instruments could not obtain similar results. There have, however, been instances in which prisms of the same kind were used by different observers, and still they pretended that their results were different. We will now try whether by a closer examination and comparison of the experiments made by those natural philosophers,—to which we may also add those made subsequently by Wünsch, Berard, and Ruhland,—a reconciliation of the existing contradictions may not be obtained.

But before doing so I will class together the chief results of my experiments.

1. An increase of heat is effected in all spectrums, which is always least at the extreme limit of the violet.

2. Thence it increases progressively through the blue, green, and yellow, to the red; and

3. Attains with some prisms its maximum in the yellow, viz. in the water-prism (Experiment 39); and, according to M. Wünsch's experiments, also with prisms filled with spirits of wine and oil of turpentine.

4. Some liquids, viz. a perfectly clear solution of sal ammoniac and corrosive sublimate, and likewise concentrated and colourless sulphuric acid, had the maximum of heat between the yellow and the red,—in the orange. (Experiments 40—43.)

5. Prisms of crown-glass and common white glass have the greatest heat in the full red. (Experiments 1—5, 20, 21, &c.)

6. With some prisms the maximum of heat falls in the limit of the red, and these seem to contain lead. (Vide Experiments 24—30.)

7. Prisms of flint-glass have the maximum of heat beyond the red when the bulb of the thermometer stands outside the well defined spectrum. (Experiments 6—18, and 31.)

8. The heat regularly decreases beyond the red, and with all prisms without exception; evolution of heat is yet found a few inches below the limit of the red. (Experiments 2, 3, 7, &c.)

In the two first positions, all observers agree; but not so in the others. Many have even thought it superfluous to mention of what prisms they made use, no doubt thinking that one spectrum would be like the other in effect. Thus Herschel has not said of what species of glass the prism consisted with which he made the discovery of the maximum of heat beyond

beyond the red: nor are we told what relation this prism bears to the others which Herschel examined. He merely says (Phil. Trans. 1800, p. 442) that he also made experiments with prisms of white glass, crown-glass, flint-glass, and water; and that "he found with all of them invisible heating rays beyond the visible red rays." He then merely mentions a few instances of heat half an inch and an inch beyond the red, but not of that in the red itself; thus leaving it uncertain where the maximum of heat was found with these prisms. Since Herschel was not sparing in the communication of his experiments, he would not have withheld any comparative experiments he had made on those prisms respecting the heat in the two places mentioned. I therefore suppose that he contented himself with simply investigating the heat beyond the spectrum, concluding that the effect would be the same in all the rest of the spectrum as it was with his first prism. It would have been very desirable that he should have explained himself on this head, as well as on the quality of his first prism, which I am inclined to suppose to have been of flint-glass.

Nor did Herschel in his other investigations place the thermometer in all the prismatic colours; but only into the violet, green, and red, and beyond the red, and once also beyond the limit of the violet; wherefore he missed the discovery that with some prisms the maximum of heat falls into the yellow and orange.

Sir H. C. Englefield, who confirmed Herschel's experiments, made use of only one prism; but he does not state whether it consisted of flint-glass or of any other kind of glass.

Rochon and Leslie alone expressly mention that they employed prisms of flint-glass in their experiments. But Rochon never carried his thermometer beyond the limit of the spectrum; and Leslie mentions but one experiment, and that made in 1797: from which, at least, it does not appear that he then made any investigation respecting the heat beyond the spectrum; as, in fact, no one seems to have done before Herschel. Mr. Leslie, indeed, tells us that he repeated Herschel's experiments, as soon as he was informed of them, with his highly sensitive photometer; but that he did not experience the least effect either above or below, or on the sides of the spectrum. How Mr. L. with that really very sensitive instrument could find no evolution of heat beyond the red half of the spectrum, I cannot conceive. For suppose even that he should have placed the limit of the red further than Herschel, (who, however, in this respect followed Newton,) it cannot be imagined that he should have carried it several inches below the bright red,

red, without his having mentioned this circumstance. And yet it has been shown by the above experiments that even there the effect on the thermometer is still continued. To seek for any other explanation of this deviation would be useless, since Mr. L. has not thought proper to give us any details of his experiments.

M. Wünsch, who has also appeared as an opponent of Herschel, maintains in the collection of the results of his experiments*, that all his prisms gave, quite near the red edge, although outside of it, always the lowest temperature; that it was stronger in the red, and still more so in the yellow. This assertion, however, is by no means borne out by the experiments given by him. He found the maximum of heat in the yellow only, with water, spirits of wine, and oil of turpentine prisms; and it was different with the glass prisms of which he made use. The first one, stained slightly green, gave the greatest heat in the full red†; it being the same as with my prisms, Nos. 1, 4, 5, 10, 11, and 12. The second, a prism of a yellowish tint, excited the greatest heat in the glimmering light at the limit of the red‡; and a third prism, which was quite colourless, seems to have had the same effect. He mentions the latter in the same article, p. 202, without giving, however, any detail of the experiment. These two prisms, therefore, seem to have resembled in effect those of my own, designated by Nos. 6, 7, 8, and 9. Of prisms of flint-glass Mr. W. made no use. We therefore find here no further explanation respecting my 7th position; but the 3rd, 5th, and 6th are confirmed by it.

I can, however, not omit mentioning a deviating observation of Mr. Wünsch's, especially as it may be considered a confirmation of one made before by Rochon. It is, that Mr. W. remarked several times that his yellowish prism excited as great a heat in the orange as on the limit of the red, and that in such case, the heat in the red, lying between those two points, was lower§. This, however, only occurred when Mr. W. made use of a lens of five inches diameter for the purpose of concentrating the coloured light, but not when he allowed the prismatic orange to fall immediately on the thermometer, as may be seen from the comparison of his 8th and 9th experiment with the 10th. It would be worth while to investigate whether the difference resulted from the effect of the lens (which is most probable), or from other circumstances. In that case,

* *Mag. der Gesellsch. Naturf. Freunde zu Berlin.* 1 *Jahrg.* 3 *heft.* p. 203.
† Vide his 1st, 2d, and 3rd Experiment.
‡ Vide his 8th, 9th, 10th, 17th, 18th, and 20th Experiment.
§ Vide his 8th and 9th Experiment.

however.

however, comparative investigations should also be made with achromatic lenses, since the simple lenses produce coloured streaks, which are probably not without influence on the result.

M. Rochon mentions, in his *Recueil de Mémoires sur la Mécanique et la Physique*, p. 652, &c. Paris 1783, fifteen experiments on the difference of heat in the red and in the orange of his flint-glass prism; in twelve of which he found the heat between the yellow and the red (in *jaune orangé* as he calls it) greater than in the red, and less in three. He made use of an air-thermometer, but the bulb does not seem to have been blackened; which probably compelled him to use a lens, in order to obtain exact results. He also distinctly says (at p. 351) that he did make use of a lens, which he inclined and directed according to the altitude of the sun.

Among the few experiments mentioned by Sir H. C. Englefield, a similar deviation, although in a different place, is recorded: the heat in the first experiment was found less on the limit of the red than in the red; but further below the limit, greater again. But he had also concentrated the light by a lens of four inches in diameter.

As for myself, I found in no instance the heat greater in the orange than in the red; but then I never made use of a lens.

Of M. Berard's experiments I only know as much as is contained in the report which was made on them by the committee of the Institute, and in Biot's *Traité de Physique* (vol. iv. p. 602, &c.). I find it no where mentioned of what species of glass was the prism which he used, or whether he used more than one. If the latter were not the case, we must suppose that the prism he used belonged to the class mentioned in my 5th position; for M. Berard found the maximum of heat near the limit of the red, the bulb of the thermometer being, however, still covered with red light.

After M. Berard, M. Ruhland undertook experiments on the evolution of heat in the prismatic spectrum, as may be seen in his prize-work, crowned by the Academy of Berlin*. He there says that he found the place of the maximum of heat variable: with some prisms of glass (of which, however, he gives no further description), and with one made of borax, he had found the maximum to be beyond the red, with others in the red, and with several liquid bodies in the yellow. How far M. Ruhland's experiments tend to confirm mine, cannot be decided, since he has omitted to give the detail of his experiments†. Of

* *Ueber die polarische Wirkung &c.* i. e. On the polar effect of coloured heterogeneous light. Berlin 1817, p. 50.

† During my residence at Munich in 1814, I communicated to Messrs.

Of Landriani's and Senebier's experiments we know but little. Senebier says that the heat in the red was always greater than in the violet, but sometimes greater in the yellow than in the red. He seems to have used a mercurial thermometer without the bulb being blackened, whence the differences proved very small: for instance, between the violet and the red only $\frac{1}{16}$th degree of Reaumur. He says his prism was English, but whether of flint- or crown-glass, he does not state.

From all these remarks on the investigations of these natural philosophers, it will be seen that the contradictions in their observations and assertions, apparently so important, are in effect trifling; but that several results which, considered singly, seemed to contradict each other, when compared with others and arranged with similar ones serve to confirm new facts, or such as have not yet been sufficiently noticed.

I will, however, now proceed to prove by experiments (also important in other respects) that the above positions derived from my observations do not embrace all cases.

I had a prism of common white glass, which I had ground for a particular purpose on two sides. One of those surfaces was yet however sufficiently bright that, when the rays of the sun passed through it and the third side which had kept its polish, a spectrum of tolerably brilliant colours was produced. With this instrument I made several experiments. All gave the same result; the heat below the red being always greater than in the red, by 3, 5, 8 and 10 lines, according as the distance was greater or smaller, and the atmosphere more or less free from vapours. I now had the dull surface of the same prism polished, so that the refraction took place through two polished sides; and in all the experiments made with it, the maximum of heat fell as decidedly within the red, as, in the former state of the prism, it had invariably fallen without it.

The question now is: whether the limits of the prismatic red were the same in both states of the prism? I had repeatedly noticed during the use of common prisms, that when the sun was suddenly clouded, yet so thinly that the spectrum was still discernible, the latter was always smaller than when the sun shone with undiminished brilliancy. In order to convince myself whether this was also the case with the ground

Ruhland and Gehlen my experiments on crown-glass, flint-glass, and water, and requested them to examine whether the flint-glass of Benedictbayern (of which the Royal Academy in Munich possesses some excellent prisms) would produce the same result as those of English flint-glass, which I had employed. But I have not learned the result. At that time M. Ruhland had not made any experiments on the heat of the spectrum.

prism,

prism, I wetted the dull side with spirits-of-wine. I thereby obtained a brighter spectrum; but at the same time the red, received at a distance of 6 feet, immediately fell lower by 2 Paris lines than it had done before. The same circumstance, then, must have occurred with the bright prism; perhaps the limit of the red expanded in it still lower, and its centre might have fallen just on that point where with the ground prism we found the maximum of heat, but only a faint reddish glimmering, which is usually not reckoned when we fix the limit of the coloured spectrum. But I could not arrive at certainty on this point by exact measurement, for want of a proper apparatus.

From these experiments I believe I may conclude that the deepest red of the spectrum was weakened to such a degree by the ground surface that it no longer formed a fixed limit, and the spectrum necessarily appeared shorter than it does with a polished prism.

What, then, was effected here by external circumstances, must with the flint-glass prism have been produced by internal; for its polish was very good, and the colours in the spectrum very brilliant and well-defined. Nor was it deficient in that weak reddish glimmering beyond the limit of the brilliant red which is produced by every prism; indeed it seemed to me to be rather brighter with the flint-glass prism than with any other I have used. Of this glimmering, however, I shall say more below.—Whether the transfer of the maximum of heat with the flint-glass prism, into this weakly coloured glimmering, be a consequence of the greater dispersion of the colours, and consequent weakening of the last deep red, occasioned by the lead it contains, or of any other dullness in the interior, I could not discover; perhaps both causes may act in conjunction. That every dullness inside the glass does not produce the same result may be seen from Experiment 22; which was made with a prism full of small blebs, and which gave a very faint spectrum, and where the maximum of heat still fell into the red.

If we now further consider the effects of flint-glass and crown-glass, it will appear as a main result,—*That the limits of the prismatic spectrum are not confined, as usually assumed, to the termination of the bright colours, where they cease with a kind of faintly coloured edge; but that the spectrum must extend further, at least to that point where the greatest effect is produced, although no colour, or at least a very faint one, should be perceivable there to the naked eye.* That *light* is there, cannot be doubted; and in fact, as we must now add, *visible light*; since several philosophers, without respect to the increasing confusion of language, still think themselves obliged

to admit invisible rays, only, they say, that our eyes are not sufficiently strong to distinguish them. Should it be the effects perceived beyond the two ends of the spectrum which gave occasion to those invisible rays, they would be superfluous on the account alone that our eyes are more susceptible of the effects of light than all our thermometers, salts, and *leuchtsteine*. Thus *visible* light is found *beyond* the usual limits of the spectrum,—it extends even to a considerable distance, and decreases gradually; and in the same proportion we see its effect on bodies decreasing, whether that effect consist in heating, as beyond the red, or in chemical operation, as beyond the violet. We need not therefore hesitate to consider this light as the active agent in this instance; for where it is absent, all agency ceases. The assumption of distinct chemical rays proceeding from the sun has found few adherents; that of heating rays has had more. But they must stand and fall together; if we do not allow those of the one kind, the others have no better support.

I must now add a few words respecting the glimmering light beyond the spectrum. It is not only bad prisms, as M. Wünsch supposes, but the very best, which have a reflection of light above and below the spectrum. But there are two kinds of glimmering: one class, with full light and a distinct limit, is sometimes occasioned by prisms with streaks and lines in their interior, but is often produced only by an irregular dispersion of light on the edges of the prisms. Such prisms ought to be excluded from this kind of investigation, or the fault be corrected by covering the edges. The other kind of glimmering is found with all prisms without exception, even with those that give the limits of the brilliant spectrum in the most distinct manner. The glimmering below the red is of a pale red colour; and that above the violet, of a very pale violet. These weak colours (which become very distinct when concentrated by a lens) decrease as they recede from the main reflection, and ultimately vanish, together with the light,—so that no limit can be given to this glimmering.

This glimmering was observed by Newton*; but he distinctly states that in his measurement he took no notice of it, because he thought that this light proceeded from the clouds, and was irregularly dispersed. In the same manner Herschel, who quotes this passage†, takes no notice of this light or glimmering, although he found such great effects in it. Nay, even when in the place where his prism excited the greatest heat, he perceived the red colour of this glimmering by means of a

* Vide *Optices*, lib. i. propos. 2, exp. 3. † Phil. Trans. 1800, p. 319.

lens,

lens*, he thought it more probable that the invisible rays might be rendered visible by concentration, than that the light existing there should be the cause of the heat. The conviction that the solar light was confined within the limits marked by Newton, and that therefore no light could be found beyond but such as came there by accident and irregular dispersion, may have induced Herschel to leave, in his investigations of the illuminating power of prismatic colours, the space beyond them unexplored; at least we find none respecting it in his microscopical investigations. Whether therefore there was any light there, and what were its effects, remained undecided.

We may consider as one of the most important arguments in support of invisible heating rays, the result of Herschel's 18th experiment†: I must therefore say something more upon that. Herschel used in that experiment a lens of 9 inches in diameter half covered with paste; with which he received all the prismatic colours on the covered part, and allowed all the light, at a distance of 1-5th of an inch from the limit of red, to fall through that which was left open, and he assures us that no trace of light or of colour was visible on the bulb of the thermometer. That he perceived no light, no one will doubt: but may it not have happened that it escaped his observation perhaps by falling on a black bulb; or that his eye, having just been exposed to a strong light, was then less acute? If Newton, with an opening in the shutter of 1-3rd of an inch, could recognise this glimmering with the naked eye at a distance of 1-4th and 1-3rd of an inch beyond the limit of the spectrum, how much more light must there have been when Herschel made use of a considerably broad prism quite uncovered? I have found, under similar circumstances, light at much greater distances.

Herschel mentions also a great number of experiments with coloured glasses, in confirmation of his theory of heating rays. But, however interesting the facts they contain, they still seem to me to decide nothing concerning the main point in dispute. To consider those experiments here would lead us too far; perhaps I may find another occasion to return to them. I will only observe generally, that investigations of the effects of coloured glasses, or on coloured light in general, will always give unsatisfactory, and indeed confusing, results, as long as the *polar contrast* in coloured light is not taken into consideration. The influence of this contrast extends to all the functions of light; it is different in every one: no effect can therefore serve as a criterion for the other,—that upon the eye

* Phil. Trans. 1800, p. 317. † Ibid.

as

as little as any other effect; it is, on the contrary, to be noticed, that even the eye is variously active in these polar influences.

If we operate with colourless light, we see the effects on bodies increase and decrease in proportion as the intensity of the light increases and decreases. But every thing is different when the light has assumed a definite colour; then the intensity of light alone no longer decides. Glasses and coloured liquids, which allow an equal quantity of light to pass through them, and which are of equal intensity of colour, may act in quite an opposite manner when coloured lights which belong to the opposite halves of colours are compared with one another. And this is a proof that the contrast in colours discovered by Goëthe*, and which on account of its analogy with the polar power of the magnet, &c., he has denonominated *polar*, is not merely external, but that in this contrast the change which is manifested in the light is of the most essential kind.

The law, that the effect of light increases and decreases in the direct ratio of its intensity, will only hold good with coloured transparent bodies so long as the colours are of the same kind, and do not differ from one another very considerably. I say, not considerably; for the colours change, and pass into others as they become deeper (yellow turns into red, blue into violet), which in the comparison of colours of the same half must not be overlooked.

From all this we may easily learn that the maximum of any effect can only take place in a definite degree of colour and a proportionate degree of intensity of light; and that therefore intensity of light and colour must be in a definite ratio, corresponding with the effect intended, if the highest degree of effect is to be attained.

If we consider this, several apparent contradictions will be reconciled, and we shall not be surprised at finding colours of the same half differing in a certain degree in effect, and, on the other hand, colours of opposite sides sometimes acting in the same manner. This last circumstance, for example, may occur with *leuchtsteine*, which may become equally bright under glasses of a pale yellow, or of a dark blue verging upon violet. This circumstance, so far from contradicting the principle of *polar contrast*, will again tend to prove it. The explanation of it will be found in the experiments communicated by me in Goëthe's *Farbenlehre*, vol. ii. p. 705, &c.; to which I now particularly refer, as they may tend to call the public attention a

* Vide his *Beiträge zur Optick*, p. 46, No. 15. Weimar 1791; and his *Farbenlehre*. Tubingen 1816.

little

little more to the subject of polar contrast, which has as yet not been sufficiently attended to; and as they may also convince those of the polarity of light, who will only acknowledge the contrast where that which on the one side is addition becomes subtraction on the other.

That this polar contrast in coloured light is also found in the evolution of heat, all the preceding experiments, as well as those of other philosophers, have sufficiently established. In this function of light the polar contrast can only manifest itself in the greater and in the less; and thus we found the heat in one half of the spectrum greatest, and in the other least.

LXVII. *Strictures by* Robert Hare, *M.D. Professor of Chemistry, &c. &c., upon Professor* Vanuxem's *Memoir on Plumbago, Anthracite, Fused Carbon, &c. published in the Journal of the Academy of Natural Sciences for June* 1825 *.

PROFESSOR VANUXEM, in a letter to Isaac Lea, Esq., which has been lately read before the Academy of Natural Sciences, endeavours to prove that the fused products obtained by Professor Silliman were none of them carbon;—first, by analysing anthracite and plumbago; and secondly, by exposing those substances, or mahogany charcoal, severally to the compound blowpipe, which he was obliged to use, not having a deflagrator.

The analyses thus given are interesting, so far as they may afford correct views of the composition of anthracite and plumbago. The only possible bearing which they can have on Professor Silliman's experiments, is in showing, what every chemist would have anticipated, especially in the case of plumbago, that there may be some ferruginous as well as earthy matter in the minerals in question, and consequently that this matter, when exposed to intense heat, may be fused into globules. This result is confirmed by the actual production of globules from anthracite, and plumbago, on due exposure under the compound blowpipe.

The fusion, however, of some ingredients in a compound does not prove the infusibility of others. If another ingredient, subjected to ignition at the same time, be not fused, it may show that it was not to be fused under the circumstances of the experiment in question; but it does not prove that under other circumstances it would be insusceptible of fusion.

The flame of the compound blowpipe, necessarily supported

* Communicated by the author.—See Professor Vanuxem's Memoir, *supra*, p. 161.

by

by oxygen gas, is very unfit for the fusion of charcoal, which when exposed to heat and oxygen passes off in the form of carbonous oxide, or carbonic acid gas;—but the opposite is true of the ignition of the deflagrator; in producing which, oxygen has little or no agency, and with whose effects it cannot materially interfere, both on account of the excessive rarefaction, and the vapour of carbonaceous matter, produced by the extreme heat.

The fusion of plumbago by the former was readily effected by me more than twenty years ago, as may be seen in my memoir on the supply and application of the blowpipe. The same result was subsequently accomplished by Professor Silliman, and now, agreeably to the memoir before us, by Professor Vanuxem himself. According to the analysis mentioned in this memoir, in which plumbago is thus admitted to be fusible, it differs from carbon only in containing three parts, in a hundred, of iron. Upon what ground then has Professor Vanuxem been so incredulous, respecting the fusibility of carbon, as to believe more readily that Dr. Macneven had obtained from it a globule of iron, than that Professor Silliman could accomplish its fusion?

Dr. Hays stated before the Academy of Natural Sciences, at their last meeting in March, that at the time of sending to Judge Cooper the globule analysed by Mr. Vanuxem, it was represented as a product of mahogany charcoal*. Professor Vanuxem has not as yet acknowledged himself, or Dr. Macneven, to have fallen into any error in treating malleable iron as a possible *extemporaneous* product of mahogany. He has not even done me the honour of noticing the paper in which it was demonstrated that, in so treating it, he had made a mistake. We are of course to infer that he still adheres to the position that wood charcoal may yield, during a transient exposure to ignition, a globule of iron in its metallic form. Under these circumstances, it must surprise every reader that he does not by an analysis of mahogany charcoal endeavour to prove that iron exists in it in such quantity, as that such a ferruginous globule may be, in such manner, obtained from it. If the negative proof founded on his neglect to analyse this substance, on which the observations of Professor Silliman were chiefly made, be combined with the positive evidence furnished by his own analysis, (that even in plumbago, which is considered as a carburet of iron, this metal does not exist in quantity adequate to have produced a globule principally ferruginous, during a momentary ignition,) it seems to me that

* Professor Macneven has recently stated the same fact, in conversation with the editor, September 1825.

the

the late memoir of Professor Vanuxem tends to prove the fallacy of that already published by him, much more than it disproves any of the allegations of Professor Silliman.

Mr. Vanuxem justifies himself for resorting to the compound blowpipe, in order to invalidate results obtained by an instrument extremely different in its character, by saying that he has done so in obedience to a suggestion of Professor Silliman. If any evidence be requisite to prove that Professor Silliman never intended to sanction such a procedure, it may be found in the following passage, concluding his observations on the results obtained by this instrument. He says,

"I would add, that for the mere fusion of plumbago the blowpipe is much preferable to the deflagrator; but a variety of interesting phænomena, both in relation to the plumbago and the charcoal, are to be exhibited by the latter, but not by the former."

In another place he observes, "Were the diamond a good conductor, it would be melted by the deflagrator; and were it incombustible, a globule would be obtained by the compound blowpipe."

It is evidently, therefore, the opinion of the author of this passage, that carbon, even in its most incombustible state, as in that of the diamond, is still too combustible to yield globules with the instrument which Professor Vanuxem has used for that purpose.

To conclude: It appears to me that the grounds upon which the results of Professor Silliman have been assailed by Mr. Vanuxem are utterly untenable. The animadversions of his first memoir were founded on an analysis of a globule, which being proved by himself to be malleable iron, was of course erroneously treated as an *extemporaneous* product from a minute portion of wood charcoal. In the memoir now under consideration he adduces experiments performed by the compound blowpipe, in order to invalidate observations made by means of an instrument of a very different character.

So far as respects the curious and interesting phænomenon of a projection arising on the charcoal attached to the negative pole of the deflagrator, I am fully prepared to bear witness to the correctness of the description given by Professor Silliman. There has been no conclusive analytic demonstration that the excrescence which thus arises is pure carbon; and had it been supposed, or proved, to contain a minute portion of iron, it would not have surprised me.

With respect to the colourless globules resembling diamond, Professor Silliman has never treated them as carbon unquestionably;

unquestionably; and I have no evidence to offer from my experiments with the deflagrator, which has any tendency to prove that diamond globules can be produced. I am not of course disposed to deny that there is much room for scepticism on this subject. ROBERT HARE.

LXVIII. *Remarks on Mr.* BURNS's *Reply on the Double Altitude Problem, by* E. RIDDLE, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

AS Mr. Burns admits that the method which he proposed for finding the latitude is inapplicable unless the true apparent times are given, and he *now* perceives that before these times can be known the *latitude must be discovered by some other process*, he admits therefore that I was right in stating that his "proposed solution is not a solution to the problem at all,"—we are on that subject perfectly agreed.

On another point however, and that a more important one, I shall simply contrast a paragraph in Mr. B.'s former communication with one on the same subject in his reply. In his former letter, when speaking of the data on which he proposed to found his solution, he says, "All that is necessary to be known is, the time, the interval between the observations, and the altitudes; all of which, from the improved state of our chronometers and other instruments, may be known with the greatest exactness." This is *all* that he says on the subject. But in his reply he says, "When I mentioned the chronometer, I did so, not for the purpose of deducing from it the apparent time, but merely to determine the interval."

I pointed out that Mr. Burns was wrong in attributing the discrepancies between the results of calculations by his method, and the approximate method of Dr. Brinkley, to the principles of the latter method, and Mr. B. now admits that on this point he was wrong; but, by way of apology for his mistake, he says that he has discovered "that the interval given in the Doctor's example is incorrect;" or, as he afterwards varies the charge, "the data in Dr. B.'s example are incongruous."

Now this apology is a singularly unfortunate one; for the data on which the Doctor's solution is founded, are the *altitudes*, the *declination*, and the *interval*; all of which are in exact accordance with the true latitude, $27^\circ\ 59'\ 16''$. The true apparent times are 3^h and 6^h, having, as they ought, the same *difference* as $2^h\ 55^m\ 12^s$ and $5^h\ 55^m\ 12^s$, the approxi-

mate

mate times given in the problem only for the purpose of finding the interval.

In my remarks on Mr. Burns's letter I stated that "Mr. Burns altogether misapprehends the nature of the problem; for he assumes as known, not only the interval of time between the observations, but *the true apparent time of each observation.*" I noted the mistake in his assumption in *italics.* Mr. Burns, however, in referring to this remark, prints it the "apparent time and the *interval;*" and other observations in his reply show that the typographical transposition was not accidental.

Having before shown that Mr. Burns had failed in giving *any* solution to the double altitude problem, and having now shown that he has not rightly stated the cause of his mistake, I take my leave of the subject, as the peculiar method of trial and error now proposed by Mr. Burns for the solution of this problem is in no danger whatever of being adopted. Mr. Burns is right in remarking that "no name however great can sanction error," and he is fully entitled to all the consolation that can be derived from the consideration that even Newton made a mistake. I am, sir, your obedient servant,

Greenwich Hospital, 17th Dec. 1825. EDWARD RIDDLE.

LXIX. *Notices respecting New Books.*

THE Second Part of the Philosophical Transactions for 1825 has just appeared, and the following are its contents:

On the anatomy of the mole-cricket. By J. Kidd, M.D. and F.R.S. Reg. Prof. of Medicine in the University of Oxford*.—Further observations on planariæ. By J. R. Johnson, M.D. F.R.S.—On the influence of nerves and ganglions in producing animal heat. By Sir Everard Home, Bart. V.P.R.S. presented by the Society for the Improvement of Animal Chemistry.—An essay on Egyptian mummies; with observations on the art of embalming among the ancient Egyptians. By A. B. Granville, M.D. F.R.S.—On the temporary magnetic effect induced in iron bodies by rotation. By P. Barlow, Esq. F.R.S.—Further researches on the preservation of metals by electro-chemical means. By Sir Humphry Davy, Bart. Pres. R.S.—On the magnetism of iron arising from its rotation. By Samuel Hunter Christie, Esq. M.A.—Some account of the transit instrument made by Mr.

* See our present Number, p. 401.

Dollond, and lately put up at the Cambridge observatory. By Robert Woodhouse, Esq. A.M. F.R.S.—On the fossil elk of Ireland. By Thomas Weaver, Esq. M.R.I.A.—Microscopical observations on the materials of the brain, and of the ova of animals, to show the analogy that exists between them. By Sir Everard Home, Bart. V.P.R.S.—On new compounds of carbon and hydrogen, and on certain other products obtained during the decomposition of oil by heat. By M. Faraday, Esq. F.R.S. Cor. Mem. Royal Academy of Sciences of Paris, &c.—Account of the repetition of M. Arago's experiments on the magnetism manifested by various substances during the act of rotation. By C. Babbage, Esq. F.R.S., and J. F. W. Herschel, Esq. Sec. R.S.—On the magnetism developed in copper and other substances during rotation. By Samuel Hunter Christie, Esq. M.A. &c.—On the annual variations of some of the principal fixed stars. By J. Pond, Esq. F.R.S. Astron. Royal.—On the nature of the function expressive of the law of human mortality, and on a new mode of determining the value of life contingencies. By Benjamin Gompertz, Esq. F.R.S.

Just published.

The English Flora, by Sir J. E. Smith, P.L.S. Volume the third, including the end of the class *Syngenesia*.

The third and fourth volumes of Kirby and Spence's Introduction to Entomology, or Elements of the Natural History of Insects.—The volumes now published complete this valuable and interesting work.

LXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

AT the Anniversary Meeting of the Royal Society, held on St. Andrew's Day, the following gentlemen were elected Council and Officers for the year ensuing.

Of the Old Council:—Sir H. Davy, Bart.; Francis Baily, Esq.; W. T. Brande, Esq.; Samuel Goodenough, Lord Bishop of Carlisle; Davies Gilbert, Esq. M.P.; J. F. W. Herschel, Esq.; Sir Everard Home, Bart.; Capt. H. Kater; John Pond, Esq.; W. H. Wollaston, M.D.; Thomas Young, M.D.

Of the New Council:—John Barrow, Esq.; John Bostock, M.D.; Sir Astley P. Cooper, Bart.; Benjamin Gompertz, Esq.; Stephen Groombridge, Esq.; Sir Abraham Hume, Bart.; Daniel

Daniel Moore, Esq.; Richard, Earl of Mount Edgecombe; P. M. Roget, M.D.; James South, Esq.

President, Sir H. Davy; *Secretaries*, Messrs. Brande and Herschel; *Treasurer*, Mr. Davies Gilbert.

The President then announced the award of the Copley medals to Messrs. Arago and Barlow for their discoveries in magnetism; and in an eloquent address upon the occasion, gave a historical sketch of the progress of the science of magnetism from the earliest periods to the present time.

Dec. 8.—The President announced His Majesty's foundation of two annual prize medals, of fifty guineas value each, to be awarded by the President and Council of the Royal Society, to the authors of such discoveries as they may deem worthy of that distinction.

A paper was read entitled, Additional proofs of the source of animal heat being in the nerves; by Sir E. Home, Bart. V.P.R.S.

Dec. 15.—The Croonian Lecture, by Sir E. Home, was read: On the structure of muscular fibre.

Dec. 22.—Two papers, by Dr. J. Davy, F.R.S., were read, On the poison of the common toad, and On the heart of animals belonging to the genus *Rana*.

The Society then adjourned over the Christmas vacation.

LINNÆAN SOCIETY.

Dec. 6.—Read a continuation of A systematic catalogue of the Australian birds in the collection of the Linnæan Society, by N. A. Vigors, Esq. F.L.S. and Thomas Horsfield, M.D. F.L.S. The portion read at this meeting included a great part of the family *Psittacidæ*, subfamilies *Plyctolophina* and *Palæornina*.

Dec. 20.—The reading of the above Catalogue was continued.—Read also Descriptions of some new species of birds belonging to the genus *Phytoloma* Gmel., *Indicator* Vieill.; and *Cursorius* Latham, by Mr. Benjamin Leadbeater, F.L.S.

GEOLOGICAL SOCIETY.

Nov. 4.—A paper was read entitled, An Account of some Geological Specimens collected by Capt. P. P. King, in his Survey of the Coasts of Australasia; and by Robert Brown, Esq., on the Shores of the Gulf of Carpentaria, during the Voyage of Captain Flinders. By W. H. Fitton, M.D., V.P.G.S. &c.

The survey of Capt. King commenced on the north-east coast of Australia, about the latitude of 21° south, and proceeded

ceeded northward and westward, omitting the Gulf of Carpentaria previously examined by Capt. Flinders,—and southward, on the western shore, to about the latitude of 25°,—where the coast had been examined by the French expedition under Capt. Baudin. The chasm in Capt. King's specimens has been supplied by those collected on the shores of the Gulf of Carpentaria by Mr. Brown, who accompanied Capt. Flinders in his survey of the coasts of New Holland.

The land visible from the sea on the north-east coast, is in general mountainous, as far north as Cape Weymouth, between the latitudes 12° and 13° (south). A high and rocky range especially, which begins about latitude 25°, is continued northward in a direction nearly parallel to the shore for more than 150 miles without interruption. The outline and aspect of this range, and of several other groups of mountains, are irregular, and resemble those of primitive tracts: peaked summits are of frequent occurrence both on the mainland and the adjacent islands. Mount Dryander, about latitude 20° 12′, one of the chief mountains, is nearly 4500 feet high; Mount Hinchinbroke, lat. 8° 22′, is more than 2000 feet; and several other mountains on this coast are of considerable elevation.

Along this part of the coast granite has been found, in detached points, through a space of about 500 miles: and rocks of the flœtz-trap formation occur in several of the islands of the shore.

The coast-line, on the north of latitude 14°, is thrown back about 40 miles to the westward of its previous course; and, about the same point, the elevation of the land declines: the general height of the mainland about Cape York, the north-eastern point of Australia, is not more than 400 or 500 feet.

The eastern shore of the Gulf of Carpentaria, occupying a space of about 500 miles more, north to south, is very low, and very uniform in its outline. The rock on the shore, at Coens River, the only point examined upon this coast, was found to be calcareous sandstone of recent formation. The western shore of the Gulf is more broken, and of higher level; and the specimens from thence consist of granite and primitive slaty rocks; upon which repose quartzose sandstone, and conglomerate, identical in character with the rocks which are found in great abundance further to the west on this northern shore, and on the north-west coast, and also with the most ancient sandstones and conglomerates of Europe. Clink-stone, and other rocks of the trap-formation, occur among the specimens from the islands in this part of Australia. The chains of islands which form the north-western verge of the Gulf of Carpentaria,

Carpentaria, and are remarkable for the similarity of their structure and their uniform direction, appear to consist principally of quartzose sandstone and conglomerate, reposing upon primitive rocks. The level of the mainland of the north coast from about longitude 135° to Melville Island, about 131°, is in general low, and is interrupted by the streams named Liverpool and Alligators Rivers; the last of which consists, in fact, of three separate streams or branches.

The specimens from Gouldburn's Islands, on the north of this part of the coast, consist of reddish quartzose sandstone.

Cambridge Gulf, about longitude 128° and latitude 15°, is one of the most remarkable inlets on the north-west of Australia. It has been traced to more than 60 miles from the sea, between hills from 150 to 400 feet in height, which have in general flat summits, and are composed of sandstone of a reddish hue and of the same characters with that already mentioned. The specimens from Lacrosse Island, at the entrance of this Gulf, are not to be distinguished from the slaty strata of the old red sandstone which occur in the banks of the Avon, between Clifton and the Severn.

The outline of the north-west coast is remarkably broken; and the shore is studded with very numerous islands; the forms of which, as well as of the hills on the mainland, are remarkable for their flat summits. In two detached points about 70 miles apart, Port Warrender and Careening Bay, epidote has been found in considerable quantity, both crystallized in veins, and in a compact form as a component of a rock of a conglomerated and amygdaloidal structure. Prince Regent's River has nearly a rectilinear course, from north-west to south-east, for more than 60 miles; and its banks of sandstone are in some places between 300 and 400 feet high. The coast to the south-west of Prince Regent's River has not yet been completely surveyed; but several openings have been observed there, of such width as to render the existence of rivers not improbable.

The shore on the western coast is in several places covered with extensive dunes of sand, with which are associated in many instances beds and masses of a very recent arenaceous breccia, abounding in shells, concreted by carbonate of lime. This formation, which is particularly remarkable in the islands and on the shores adjacent to Shark's Bay, about latitude 25°, is analogous to that which occurs very extensively in Sicily, at Nice, and several other places on the shores of the Mediterranean, and of the West India Islands, and on many parts of the coasts within the Tropics. In New Holland it generally consists of sand cemented by stalagmitic or tufaceous carbonate

bonate of lime, containing angular fragments of a compound of the same nature, but previously consolidated and broken, along with numerous shells and fragments of shells, very nearly resembling those of the adjacent seas. Its date appears to be more recent than that of the beds which constitute the Paris and London basins; but anterior to the accumulation of the diluvial gravel, and possibly coeval with that of the crag of Suffolk, and the eastern coast of England; though the strata are in several respects different from these last-mentioned deposites.

The calcareous concretions of New Holland have in some instances a tubular and stem-like appearance, and have thence been mistaken for corals and petrified branches of trees.

On a general view of the north and north-west of New Holland, it will be observed that the outline of the coast, in several distant quarters, has a direction nearly uniform, from south-west to north-east; which is the course also of the remarkable ranges of islands on the north-west of the Gulf of Carpentaria. It appears also that reddish sandstone of ancient date is very abundant throughout the north and west coasts; and it is not altogether improbable that the prevailing direction of the strata may be that above mentioned.

So little is known of the remainder of Australia, and especially of the interior, that speculations upon its general structure would be premature; but the linearity of the coast-lines in several other places is remarkable; and their course, as well as that of the principal opening, has also a general tendency to a direction from the west of south toward the east. The coincidence of uniformity of range, with marked features of geological constitution, is of such frequent occurrence in other parts of the globe, that these appearances are in the present case deserving of attention; but they are mentioned by the author, under the existing scantiness of information from Australia, merely as suggesting ground for more extensive inquiry.

ASTRONOMICAL SOCIETY.

Dec. 9.—The President informed the Society that when he had the honour of announcing, at their last meeting, the extraordinary occurrence of the appearance of *four* comets in the short space of as many months, he was little aware that he might at that time have added a *fifth* to the number. This last comet appeared, from the account stated in the public journals, to have been discovered by M. Pons, at the beginning of last month: but, as it had considerable *south* declination and was advancing also to the southward, and at the same time very faint, it probably would not be seen in this country.

Although

Although the appearance of so many comets in one year had been mentioned as a remarkable phænomenon, yet he would not wish to be understood as supposing that such a circumstance had never previously occurred, nor was likely to occur again. The fact was, that from the great attention which had been paid by astronomers to the discovery of these bodies within these few years, and the interest excited by the investigation of the laws by which they were governed, a more than ordinary diligence had been employed in searching for them. And there was every reason to believe, that if there were more labourers in the field, a still richer harvest would ensue: from which there might fairly be expected some additional light on the laws and constitution of the universe.

The President likewise called the attention of the members to the circumstance of the opposition of Mars in the month of May in the ensuing year. It was well known, he remarked, that by a comparison of the observations of this planet with the stars which were near it at that time, made at places situated in these latitudes and at other latitudes having considerable southern declination, the parallax of the planet might be readily deduced, and thence the parallax of the sun. As there were, at this time, two active observatories in the southern hemisphere, where this phænomenon would probably be attended to (as it had been at the two preceding oppositions), it were extremely desirable that corresponding observations should be made in the northern hemisphere; without which, the observations made in the south would (as far as this subject is concerned) be rendered of little or no use. He trusted, therefore, that those practical astronomers who were possessed of the requisite instruments (and they were by no means complex or expensive) would attend to this phænomenon, and record the observations which they might have the advantage and opportunity of making; the uncertainty of this climate rendering it extremely desirable that all those, who had the means, should unite in so useful an undertaking.

For the convenience of such observers, the President announced that he had computed the right ascension and declination of six stars, near which Mars would pass a few days before and after his opposition; these being the whole which he could find in any of the catalogues. They were here offered only as a mean of identifying the star with which the planet may be compared. It was probable that other stars might be seen, in the field of view of the telescope; and that even some of these might not be found: for the catalogues of the smaller stars are still very imperfect. It would render observations of this kind more complete and useful, if regular observations of

such stars as might be situated near Mars at the time of his opposition, were made at the public observatories: whereby the true position of the planet in the heavens would be more correctly ascertained.

The following are the mean positions of the stars above alluded to on the 1st of January 1826.

Star.	Mag.	Æ.	D.
		h m s	° ′ ″
8 Libræ	6	14 41 5	−15 16 2
α ——	3	41 16	15 18 42
(195) P	8·9	43 0	15 40 36
L. L. X	7	46 38	16 5 12
(252) P	8·9	53 51	15 54 8
ν^2 ——	6·7	57 7	15 48 13

The reading of the description of the large reflecting telescope and frame made by Mr. John Ramage, of Aberdeen, was terminated. Mr. Ramage has, ever since the year 1806, devoted much of his time to the construction of reflecting telescopes of large size, and of convenient frames and supports, in which firmness of structure and facility of adjustment to any required position, should be equally attained. The telescope now described has a twenty-five feet tube. The platform upon which the telescope is placed, and revolves at pleasure, is a strong circular rail-way of cast iron, twenty-seven feet and a half in diameter, and four inches in breadth. The horizontal azimuthal motion is upon concentric rollers, round a central pivot. The stand or frame, though simple in its construction, cannot be very intelligibly described without a model or a diagram. The tube of the telescope is elevated to the required altitude by a winch and tackle of pulleys. The gallery in which the observer stands is adapted to the proper height by a similar winch and tackle; and to prevent accident from the breaking of the ropes, it is supported at each side by two moveable bars that fall into the steps of the ladders, which constitute a part of the frame. The lower end of the tube rests upon two rollers, and at great altitudes moves forwards, so that the tube itself is capable of adjustment to all positions, from that which is nearly horizontal to that which is nearly vertical. Without quitting the gallery, the observer can move the tube both horizontally and vertically upwards of 10°, and can with the utmost readiness (independently of an assistant) direct the telescope to any point in the heavens. All the motions are effected by means of a very

very few cords, pulleys, and winches. The diameter of the speculum is fifteen inches, and the focal length twenty-five feet. The eye-pieces, which are adapted to magnify the image, possess powers varying from 100 to 1500; and there are proper diaphragms to modify the redundancy of light. The mode of observing is by the "front view."

Mr. Ramage exhibited to the Society, besides a neat model of the tube and apparatus, two speculums; one of fifteen inches diameter, belonging to the telescope described, and another of twenty-one inches diameter and fifty-four feet focus.

There was next read a paper on the subject of Parallaxes, taking the word in an enlarged sense, by M. Littrow. It was in the excellent treatise of Lagrange on the determination of the solar parallax, from the observed transits of the inferior planets over the sun's disc, where the rectangular co-ordinates were first employed, instead of the less convenient expressions of spherical trigonometry, for the purpose of deducing the apparent station of a planet from its longitude and latitude. The process has been since improved by Olbers, Bessel, Rhode, &c. But M. Littrow regards it as susceptible of still further improvements, which he has here exhibited. He gives the analytical solution of several problems; viz.

1. To determine the apparent longitude and latitude of a star, from the true geocentric longitude and latitude.

2. To solve the inverse problem.

3. and 4. The solution of the preceding problems by series.

5. To find the apparent right ascension and declination, from their true magnitudes, and *vice versâ*.

6. To determine the apparent azimuth and altitude, from their true magnitudes, and *vice versâ*.

7. and 8. To find the true place of the star, from its apparent place, and *vice versâ*, without any reference to the horizon, the ecliptic, and the equator, which is often useful in computing the occultation of fixed stars by the moon.

9. A general problem, to find the apparent azimuth and apparent altitude, from the true longitude and the true latitude of a star.

The resulting expressions for these several solutions are analytically simple. Those which are deduced in series are usually of this kind, namely,

$$\log c = \log b - \left(\frac{a}{b}\right)\cos\theta - \tfrac{1}{2}\left(\frac{a}{b}\right)^2\cos 2\theta - \tfrac{1}{3}\left(\frac{a}{b}\right)^3\cos 3\theta, \text{ \&c.}$$

in which the *law* is evident.

M. Littrow concludes his paper by suggesting the applica-

tion of his principal formulæ to the solution of various other problems.

Lastly, there was read a paper entitled A memoir on different points relating to the theory of the perturbations of the planets expounded in the *Mécanique Céleste*; by M. Plana, astronomer royal at Turin and an associate of this Society.

The object of the author in this memoir he states to be an examination of various points in the theory of the planetary perturbations as explained by M. de Laplace in the *Mécanique Céleste*. In undertaking this labour, he observes, he at first had no expectation of meeting with any instance in which an actual rectification of the results already arrived at would be necessary; but the progress of late made in the theory of perturbations having enabled him to treat certain particular questions more generally, and with more symmetry than heretofore, it is not to be wondered at if he has been led to results which surpass in exactness those hitherto published. But in all such cases, he adds, where he has arrived at conclusions not in accordance with those of the illustrious author of the *Mécanique Céleste*, he has thought it incumbent on him to give with the fullest detail, not only the developments, but even the arithmetical calculations on which these conclusions have been founded.

The 1st chapter is devoted to the consideration of that artifice in the *Mécanique Céleste* in which M. Laplace transfers his formulæ from the mean motions, axes, &c. of the primitive or undisturbed orbits, which are not given by observation, to those of the disturbed, which are given as they exist in nature. This he does by assuming an arbitrary constant introduced in one of the integrations by which the perturbation in longitude is derived, in such a manner as to make the term in the result which depends on the mean motion vanish. M. Plana devotes this chapter to the elucidation of this artifice, and shows the correctness of M. Laplace's results by obtaining the same conclusion by another, and direct method. He then applies his reasoning to numerical examples, and computes the quantity by which the moon's mean distance from the earth is *permanently* altered by the sun's action, which he finds to be about 1-100dth of the radius of the globe of the moon, in augmentation, the corresponding increase of the periodic time being about 1-4th of a day. The excentricity too undergoes an alteration in its *mean* quantity from the same cause, equal to about 0·0007 of its actual amount.

A similar artifice in the use of an arbitrary constant added in one of the necessary integrations for arriving at the first term of the motion of the moon's perigee, M. Plana observes,

has

has enabled M. Laplace to avoid an error in that research to which his method seemed to expose him, and to obtain the true result. But he proceeds to show that this artifice is not necessary, and that the same result may be obtained without the use of the superfluous constant, by the aid of an equation he deduces for the variable portion of the moon's radius vector.

The method employed by M. Plana has the advantage, he observes, of keeping distinctly in view throughout the whole analysis the primitive elements, uninfluenced by the effect of perturbation. The other he states to have been first employed by Lagrange in the volume of the Memoirs of the Academy of Berlin for 1783.

The author next proceeds to examine those parts of the theory of perturbations, which depend on the non-sphericity of the central body, and in which he remarks that the use of a similar artifice in the *Mécanique Céleste* is accompanied with greater obscurity, as a portion only of the arbitrary constant is retained. He therefore enters on the investigation without the use of this artifice, and deduces the results for the perturbations of the planets due to the ellipticity of the sun by the formulæ for the variation of the arbitrary constants.

The author next applies the same method to the theory of the perturbations of the seventh satellite of Saturn by the elliptic figure of the planet; and as he here arrives at final equations somewhat differing from those of M. Laplace, the whole process is given in copious detail.

The 2d chapter of this paper is devoted to the consideration of the effect of the actions of the fixed stars on the secular variations of the planetary system. The expressions for the secular variations of the excentricity and aphelion which the author brings out, agree perfectly with Laplace's in form, but differ in the numerical coefficients, one of the terms having the coefficient $-\frac{15}{4}$, where Laplace has $\frac{3}{4}$, and another $-\frac{3}{2}$ where Laplace makes it -1. As he subsequently observes however, the action of the stars cannot possibly become sensible till after the lapse of many hundreds of centuries; so that these discrepancies are practically of no importance. He remarks too that this cause of perturbation prevents the equations between the squares of the excentricities, the masses, and square roots of the axes, so often referred to as insuring the stability of the planetary system,—as well as the similar one between the squares of the tangents of the inclinations, the masses, and square roots of the axes,—from being mathematically exact. It will be noted, however, that these equations can only be regarded as proved for the first powers of the disturbing forces, while

while the action of the stars is at least of the order of their squares or even cubes.

The 3rd chapter is devoted to the evaluation of those terms in the theory of the perturbations of Mercury by the Earth whose coefficient, being divided by the square of the difference between the mean motion of Mercury and four times that of the Earth, may acquire a notable value by the smallness of its divisor. The author first examines the indirect method followed by M. Laplace, which he considers defective and in some measure illusory, and then substitutes a method of his own. After going through all the very laborious calculations of the analytical and numerical values of the coefficients, he arrives at a final result, of which he remarks that although it differs very little from that given in p. 98 of the third volume of the *Mécanique Céleste*, and in p. 32 of the tables of Mercury published by M. Lindenau, yet this apparent accordance is merely a consequence of the excessive smallness of the numerical coefficient of the term in question, and that his object has rather been to rectify the analytical formulæ than the numerical results, by taking into consideration *all* the terms of the same order; without which he considers it very possible to commit material errors in the final results of such operations.

The 4th chapter has for its object an examination of M. Laplace's method of taking account of the square of the disturbing force in the theory of the great inequality of Jupiter and Saturn.

In this investigation the author is led to conclude, that the equation connecting the reciprocal perturbations of the mean motions of two planets, and by which the one may be derived from the other by a simple multiplication, holds good only when the first powers of the disturbing forces are considered (a consequence, it may be observed, one might naturally presume from the form of the multiplier itself, into which the simple ratio of the masses only enters as a factor).

$$\left[\zeta' = -\frac{m}{m'}\sqrt{\frac{a}{a'}}\,\zeta_{,}\right].$$

M. Plana gives this part of his paper with the fullest possible detail, in order, he observes, to enable astronomers to verify every part of the developments and calculations; and on reducing his formulæ to numbers, obtains (not, as he says, without surprise) a final result, of a contrary sign to that of Laplace, and only one third of its amount, the coefficients of the terms of the great inequality arising from the square of the disturbing force being according to M. Plana

$-\ 1''{\cdot}9200$ and $+\ 5''{\cdot}5775$ for Jupiter
$+25''{\cdot}1036$ and $-12''{\cdot}8932$ for Saturn.

The

The 5th chapter contains reflections on the Supplement to the theory of Jupiter and Saturn in the fourth volume of the *Mécanique Céleste* (p. 327—344); in which M. Laplace considers several terms of the order of the square of the disturbing force arising from the variation of the excentricities and perihelia of the two planets, affected by the argument of the great inequality. M. Laplace has made use of an indirect but more expeditious method; and the object of the author in this chapter (admitting, however, that the indirect method cannot fail to give results very near the truth) is to estimate their degree of accordance with those afforded by the direct method. His conclusions in a numerical point of view agree with those of Laplace, but he conceives that his analysis is more rigorous and his formulæ better adapted to further developments.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Aug. 1.—M. Surun recited some observations on the plague, which he has inserted in his new elements of pathologic physiology.—M. Hachette communicated some observations on curves of the second degree.—The death of M. Vassali-Eandi, a correspondent of the Academy at Turin, was announced.—M. Arago stated that the Encke comet had reappeared and had been observed by M. Pons, at the observatory of La Marlia.—He also communicated some new thermometrical observations made at different depths during the late heats.—MM. de Laplace, de Prony, and de Rosel presented a favourable report on the memoir of MM. Nicollet and Brousseaud, entitled, Exposition of operations relative to the measurement of an arc of mean parallel between the pole and the equator.—M. Geoffroy St. Hilaire read an extract from a work on human monsters characterized by the absence of the arbrospinal marrow, and named anencephala.

LXXI. *Intelligence and Miscellaneous Articles.*

OBITUARY.—Samuel Parkes, Esq.

DIED on the 23d December, at his house in Mecklenburg-square, after a lingering illness, Samuel Parkes, Esq. F.L.S. &c. &c. author of The Chemical Catechism, Chemical Essays, Rudiments of Chemistry, and other works.

Mr. Parkes was born at Stourbridge in Worcestershire, and received his education at the academy conducted by Dr. Addington at Market Harborough.

The benevolence of his disposition and the amenity of his manners attached to him a large circle of friends; and in him the community have lost a most estimable member. His works

works attest his ardour, diligence, and perseverance in the pursuit of science; nor was he less distinguished by his beneficent efforts and pecuniary liberality in the support of every public institution which tended to increase the happiness or promote the improvement of his fellow-creatures. His industry and activity of mind were evinced even during his last illness, by his being anxiously engaged in preparing and superintending improved editions of his chemical works.

LIST OF NEW PATENTS.

To Augustus Count de la Garde, of St. James's-square, for improved machinery, communicated from abroad, for breaking or preparing hemp, flax, &c.—Dated 24th Nov. 1825.—6 months to enrol specification.

To Joseph Eve, of Augusta Georgia, in America, but now residing at Liverpool, for his improved steam-engine.—24th Nov.—6 months.

To Henry King, of Norfolk-street, Commercial-road, master mariner, and William Kingston, of Portsmouth Dock-yard, master millwright, for improved fids for top masts, gallant masts, bowsprits, and all other masts and spars to which the use of the fid is applied.—26th Nov.—6 months.

To Richard Jones Tomlinson, of Bristol, for his improved frame-work for bedsteads and other purposes.—26th Nov.—6 months.

To Marc Lariviere, of Prince's-square, Kennington, Surry, for his apparatus to be applied to the stamps, fly-presses, or other presses for perforating metal plates, and for the application of such perforated plates to various useful purposes.—28th Nov.—6 months.

To William Pope, of Ball-alley, Lombard-street, mathematician, for his improvements on wheeled carriages.—3d Dec.—6 months.

To William Pope, of Ball-alley, Lombard-street, for improvements in making, mixing, improving, or altering soap.—3d Dec.—6 months.

To Henry Berry, of Abchurch-lane, for his improved method of securing volatile or other fluids, and concrete or other substances, in various bottles and vessels.—3d Dec.—6 months.

To Ezekiel Edmonds, of Bradford, Wiltshire, clothier, for improvements on machines for scribbling and carding sheep's wool, cotton, or any fibrous articles requiring such process.—3d Dec.—6 months.

To John Beever, of Manchester, for an improved gun-barrel.—3d Dec.—6 months.

To Edmund Lascombe, of East Stonehouse, Devonshire, for a method, communicated from abroad, of preparing an oil or oils extracted from certain vegetable substances, and the application thereof to gas light and other purposes.—6th Dec.—6 months.

To John Phillips Beavan, of Clifford-street, Middlesex, for a foreign invention of a cement for building, &c.—7th Dec.—6 months.

To Francis Halliday, of Ham, Surry, esquire, for improvements in machinery to be operated upon by steam.—9th Dec.—6 months.

To Joseph Chesseborough Dyer, of Manchester, patent card manufacturer, for improvements in machinery for making wire cards for carding wool, cotton, tow, &c., and also certain improvements on a machine for shaving and preparing leather used in making such cards.—9th Dec.—6 months.

To Robert Addams, of Hammersmith, for a method of propelling carriages on turnpike, rail, or other roads.—14th Dec.—6 months.

To Matthew Ferris, of Longford, Middlesex, calico-printer, for his improvements on machinery for printing cotton, &c.—14th Dec.—6 months.

To James Ashwell Tabor, of Jewin-street, Cripplegate, for his means for indicating the depth of water in ships and vessels.—14th Dec.—2 months.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. BURNEY at Gosport, Mr. J. CARY in London, and Mr. VEALL at Boston*

	Days of Month, 1825.	Gosport, at half-past Eight o'Clock, A.M.							Clouds.							Height of Barometer, in Inches, &c.		Thermometer. London.			Thermometer. Boston.	Rain.		Weather.		
		Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Nov.	26	30·15	45	53·15	91	SW.	...	0·020	...	...	1	...	...	1	1	30·62	29·70				44	...	...			S.
	27	30·00	44	.....	86	W.	0·08	·150	1	1	1	...	...	1	1	29·90	29·55				41	...	0·07			SW.
	28	29·42	51	.....	91	SW.	...	·500	1	...	1	...	1	1	1	29·20	28·95				52	...	·26		Rain *p.m.*	SW.
	29	29·00	46	.....	85	SW.	...	·450	...	...	1	...	...	1	1	28·96	28·65	44	45	43	43·5	...	...	Rain		S.
	30	29·43	41	.....	82	SE.	·07	·300	1	1	1	...	1	1	1	29·72	29·15	40	41	38	38	...	·19	Fair	Rain *a.m.*	NW.
Dec.	1	29·60	38	52·50	85	SE.	...	·485	1	1	1	...	1	1	1	29·60	29·45	33	38	40	54	...	...	Fair		SW.
	2	29·00	41	.....	90	W.	...	·420	...	...	1	...	1	1	1	29·06	28·80	44	45	42	40	...	·26	Fair		SE.
☾	3	29·28	43	.....	86	SW.	·09	·480	...	...	1	...	...	1	1	29·34	28·92	40	43	40	37·5	...	·20	Fair		SW.
	4	29·10	40	.....	86	NE.	...	·500	1	...	1	...	...	1	1	29·22	29·00	46	48	46	32	...	...	Rain		W.
	5	29·26	39	.....	88	NE.	...	·410	1	...	1	...	...	1	1	29·28	29·23	45	46	47	31	...	...	Cloudy		W.
	6	29·40	41	52·10	94	SW.	·11	.....	1	...	1	...	...	...	...	29·47	29·10	48	51	47	44	...	·64	Fair	Rain *a.m.*	SW.
	7	29·22	48	.....	90	SW.	...	·090	...	...	1	...	...	1	1	29·22	29·05	43	49	47	42·5	...	...	Fair		SW.
	8	29·34	44	.....	89	NE.	...	.....	...	...	1	...	...	...	...	29·42	29·22	45	44	45	44	...	...	Cloudy		SW.
●	9	29·40	43	.....	86	NE.	·14	.....	...	...	1	...	...	...	1	29·53	29·20	44	45	45	44	...	·05	Cloudy	Rain *a.m.*	SW.
	10	29·67	42	.....	86	NW.	...	.....	...	...	1	...	...	...	...	29·75	29·40	44	45	45	46	...	...	Cloudy		W.
	11	29·75	40	.....	87	NW.	...	·005	1	...	1	1	1	...	1	29·80	29·50	42	48	44	45	...	·19	Cloudy		SW.
	12	29·88	42	51·95	87	N.	·12	·010	...	...	1	...	...	1	1	29·90	29·57	42	41	40	40	...	...	Cloudy		S.
	13	29·82	36	.....	89	SE.	...	·930	1	...	...	1	...	1	1	29·79	29·45	35	44	48	38·5	...	·39	Foggy	[at night.	S.
	14	29·40	48	.....	92	S.	...	·250	...	...	1	...	...	1	1	29·33	29·10	48	50	42	47	...	·26	Rain	Lightning with rain	S.
	15	29·58	37	.....	86	NW.	·07	·180	1	...	1	...	1	1	1	29·76	29·20	38	40	45	31	...	...	Fair		SW.
	16	29·74	49	.....	84	W.	...	·170	1	1	1	...	1	1	1	29·84	29·32	51	54	49	50	...	·18	Fair		SW.
☽	17	29·72	52	.....	94	SW.	...	·170	1	...	1	...	...	1	1	29·83	29·40	50	51	49	47	...	·04	Fair		SW.
	18	29·64	49	51·80	91	S.	·09	·160	...	...	1	...	...	1	1	29·70	29·35	50	54	48	45·5	...	·05	Cloudy	Rain at night	S.
	19	29·32	48	.....	89	S.	...	·110	1	1	1	...	1	1	1	29·43	29·03	49	48	46	48·0	...	·04	Fair	Hail and rain *p.m.*	SE.
	20	29·32	45	.....	86	SE.	...	·045	1	1	1	...	1	1	1	29·47	29·22	46	47	49	41·5	...	...	Fair		SE.
	21	29·44	48	.....	90	SE.	·08	·410	...	...	1	...	...	1	1	29·57	29·22	49	51	50	45	...	·04	Fair		S.
	22	29·48	41	.....	89	NW.	...	.....	1	1	1	...	...	1	..	29·66	29·22	49	48	43	48	...	...	Cloudy	Rain *a.m.*	S.
	23	29·80	37	.....	92	NW.	...	·160	1	1	1	...	...	1	1	29·87	29·55	38	46	43	38	...	...	Cloudy	Rain *p.m.*	W.
	24	30·08	36	.....	93	NW.	...	·060	1	1	1	...	...	1	1	30·15	29·70	35	40	39	40	...	·05	Fair		SW.
○	25	29·80	48	51·60	96	SW.	·12	.....	1	1	1	...	...	1	1	29·70	29·40	46	51	44	48	...	·07	Fair	Rain *a.m.*	S.
Aver. :		29·535	43·40	52·18	88·6		0·97	6·465	18	10	29	2	9	25	26		29·25				43·0		3·00			

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